Channeling Contention

Political Institutions and the Onset of Nonviolent Uprisings in Authoritarian Regimes

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Master's Thesis

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Abstract

Why do we see such high levels of civil resistance in authoritarian regimes? Recent studies indicate that the vast majority of nonviolent uprisings take place in dictatorships (Chenoweth and Lewis 2013b; Chenoweth and Stephan 2011). An issue that has been offered scant attention in this debate is the considerable institutional diversity that exists among authoritarian regimes. This thesis sets off to explore this topic and clarify the effects of political institutions on nonviolent uprisings. Based on previous work on authoritarian institutions and survival, I identify the effects restrictions on political parties as a particularly fruitful subject for investigation. I argue that the level of restrictions on political parties in dictatorships serves as a key determinant of the opposition's willingness to pursue regime change through conventional channels of participation, as opposed to nonviolent direct action. This 'channeling effect' is neither linear nor constant. Dictatorships face varying levels of opposition and differ in their ability to co-opt opposition. Moreover, dictatorships experience different patterns over time, depending on the type of institutions that are established. As such, authoritarian regimes should exhibit predictable differences in their ability to prevent nonviolent uprisings.

Using data on nonviolent uprisings between 1973 and 2006, I run a series of logistic regression models to test these claims empirically. I explore the effects of three different levels of institutionalization - no parties, a single party or several parties - on the risk of a nonviolent uprising. I find that single-party regimes run a significantly higher risk of a nonviolent uprising than both no-party and multiparty autocracies, while no-party regimes and multiparty autocracies face virtually the same likelihood of an uprising. Furthermore, I find that the effect of regime duration is significantly higher in multiparty autocracies than in other autocracies. While the risk of an uprising decreases over time in no-party regimes, and remains fairly stable in single-party regimes, this risk increases significantly over time in multiparty autocracies. Thus, political institutions do appear to channel contention, but their ability to do so is highly conditional.

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I alone am responsible for the errors in this thesis.

Haakon Haugevik Jernsletten
Oslo, May 2015
Word count: 41519

Replication data for the analysis are all made available to the public.[1] More information regarding the dataset and do-files from STATA or the R-script can be provided upon request.
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[1] https://www.dropbox.com/sh/2994k6u926ytibu/AACRUd6oehufBJZICJCTe0fva?dl=0

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1 Introduction

> *Montesquieu observed that, at birth of new polities, leaders mold institutions, whereas afterwards institutions mold leaders.*
>
> Robert Putnam (1993: 26).

More than four years have passed since the wave of popular uprisings that hit the Arab world in 2011. Ever since, multiple reporters and scholars have attempted to explain the rapid unraveling that took place during the so-called Arab Spring. Yet, even with the benefit of hindsight, significant confusion remains as to how the revolutions could unfold (Chenoweth and Stephan 2014). After all, the uprisings brought down some of the most entrenched and repressive dictatorships in the Middle East (Svolik 2012).

This brings me to the general empirical puzzle motivating this thesis: why do we see such high levels of civil resistance in authoritarian regimes? As Erica Chenoweth and Maria Stephan (2011: 66) have demonstrated in their seminal book, "Why Civil Resistance Works", the "vast majority of nonviolent campaigns have emerged in authoritarian regimes […] where even peaceful opposition against the government may have fatal consequences". However, despite the fact that later studies have reaffirmed this finding, none have systematically addressed its implications or explored this pattern any further (Butcher and Svensson 2014; Chenoweth and Lewis 2013b; Cunningham 2013). What I suggest here is that the finding merits further exploration given more recent insights on authoritarian regimes.

In particular, a burgeoning literature on authoritarian regimes has highlighted the considerable institutional heterogeneity among dictatorships. As Gandhi and Przeworski (2006: 1) contend,

> Dictatorships are not all the same. Some are purely autocratic: a single man or a clique rules, unconstrained by any institutions. In contrast, some dictatorships exhibit the full panoply of seemingly democratic institutions.

Several scholars have come to believe that authoritarian regimes are categorically different in terms of structure and behavior, and that these differences have empirically observable implications (e.g. Geddes 1999; Hadenius and Teorell 2007; Svolik 2012). Here, I mean to build on these insights, and investigate whether institutional differences among dictatorships affect the likelihood of nonviolent uprisings.

The broader subject of this thesis is therefore the effect of political institutions on the mobilization of nonviolent uprisings. Specifically, I wish to address the effects of restrictions on political parties on the risk of nonviolent campaigns. By restrictions on political parties, I refer to the distinction between dictatorships that permit no parties, a single party, or several parties. I label these regimes *no-party regimes*, *single-party regimes*, and *multiparty autocracies*, noting that some authors (e.g. Geddes 1999) also use the term single-party regimes to capture all dictatorships with dominant parties. Each of these three levels of restrictions of political parties represents different levels of institutionalization (low to high).

Significant scholarship has addressed the inner and outer workings of single-party regimes (e.g. Geddes 1999; Magaloni 2008b; Smith 2005), as well as the dynamics of competitive autocracies (e.g. Diamond 2002; Levitsky and Way 2002; Schedler 2002b). Recent studies have also started to address the effects of political party restrictions on outcomes such as coup d'états, repression, and popular uprisings (e.g. Frantz and Kendall-Taylor 2014; Gandhi 2008; Svolik 2012). Thus, there is already a considerable literature that I can lean on that appears to carry particular relevance for nonviolent campaigns. Also Chenoweth (2015: 367) has noted the potential relevance of restrictions on political parties for nonviolent mobilization, labeling it "an area ripe for future research". Thus, I mean to explore the following research question:

Research question: *What is the effect of restrictions on political parties on the likelihood of nonviolent uprisings in authoritarian regimes?*

Overall, the argument presented here is that the level of restrictions on political parties serves as a key determinant of the opposition's willingness to pursue regime change through conventional channels of participation, as opposed to nonviolent direct action. This argument stems from the observation that dictators typically rely on greater levels of institutionalization to counter stronger opposition movements (e.g. Gandhi and Przeworski 2006). More specifically, I postulate that the effect of permitting more parties is neither linear nor constant. Instead, it is conditioned on two factors: (1) the threat of internal disruptions within the ruling coalition, and (2) the proximity to the last regime change. Hence, I identify single-party regimes as particularly prone to nonviolent uprisings because the internal power-sharing dynamics of these regimes make them liable to 'underinstitutionalize' in the face of stronger popular opposition. Conversely, I identify multiparty autocracies as particularly apt at co-opting key segments of the opposition that otherwise would be inclined to engage in

nonviolent action. Crucially, this feature of multiparty autocracies makes them no more likely to experience uprisings than no-party regimes, despite the fact that no-party regimes typically face weaker opposition movements. Finally, I suggest that the stabilizing properties of permitting multiparty autocracies should decline over time. As such, the effects of regime duration should be higher in multiparty autocracies than in other autocracies, which tend to experience more instability at the initial stages of their tenure. The general implication of these arguments is that dictatorships should exhibit predictable differences in their ability to avoid nonviolent uprisings.

In my empirical analysis I find significant support to back all of these claims. The results show that single-party regimes experience a significantly higher risk of nonviolent campaign onsets than both multiparty and no-party autocracies, and the differences appear to be quite substantial. Furthermore, the risk of a nonviolent campaign onset is virtually identical in no-party and multiparty autocracies, and remains insignificant throughout all of the model specifications. Finally, the results also reveal that the effect of regime duration of nonviolent uprisings is higher in multiparty autocracies than other autocracies. Whereas the risk of a campaign onset decreases over time in no-party regimes, and remains relatively stable in single-party regimes, multiparty autocracies experience a much higher risk of campaign onset over time.

The aim of this thesis is to contribute to a growing literature on the onset of nonviolent campaigns (e.g. Chenoweth and Ulfelder 2015; Dahl, Gates, Gleditsch, and González 2014). Studying the initiation of nonviolent campaigns is valuable for several reasons. First, nonviolent campaigns have been shown to be highly successful in removing incumbent governments from power (Chenoweth and Stephan 2011). Compared to armed campaigns, nonviolent uprisings are much more likely to lead to democratic transitions, democratic consolidation and subsequent periods of civil peace (Celestino and Gleditsch 2013; Chenoweth and Stephan 2011). Moreover, popular revolts have also become increasingly common the last few decades, and now constitute the most prevalent mode of nonconstitutional leader exit in authoritarian regimes (Kendall–Taylor and Frantz 2014). Thus, the potential of nonviolent campaigns to usher into more profound political change justifies an attempt to explain them.

1.1 Central Concepts

To facilitate reading, I will first clarify the main terminology of the thesis. These concepts include: nonviolent resistance campaigns, political regimes, political institutions, and dictatorship.

1.1.1 Nonviolent Resistance Campaigns

The substantive focus in this thesis is on the onset of nonviolent campaigns over regime change. Here, I will use the terms nonviolent campaigns, civil resistance campaigns, nonviolent uprisings, nonviolent conflict and unarmed insurrections interchangeably. These events are forms of non-routine direct action, which denote activities outside normal political channels and in violation of what is sanctioned by the state (Celestino and Gleditsch 2013; Sharp 1973). They are nonviolent in the sense that they are performed by unarmed civilians, who do not use or threaten to use physical force (Chenoweth and Lewis 2013: 418). They can encompass both acts of omission, whereby people refuse to do something the authorities' orders require (e.g. tax boycotts and sit-ins), and acts of commission, whereby people do something authorities seek to prevent (e.g. strikes or illegal demonstrations) (Sharp 1973).

By campaigns, I refer to "a series of observable, continuous, purposive tactics or events in pursuit of a political objective" (Chenoweth and Stephan 2011: 14). Campaigns are therefore distinct from protests, which are often singular events and represent only one of the many tactics that may be represented in nonviolent campaigns. They are typically coordinated events, organized by a central leadership comprised of activists, public figures, and civilians (Chenoweth and Ulfelder 2015: 2). In the present inquiry I focus in particular on campaigns that aim to overthrow the existing regime. These campaigns pursue what Chenoweth and Stephan label maximalist goals, and constitute some of the most intense forms of civil resistance.

1.1.2 Political Regimes and Political Institutions

Following Bratton and van de Walle (1997: 38), I define political regimes as "the sets of procedures that determine the distribution of power." [2] I capture differences among regime types by in first instance differentiating between democracies and authoritarian regimes, and

[2] Note, however, that I will occasionally use "the regime" to refer to the incumbents rather than regime types.

then by separating autocracies according to some of their most prominent institutional attributes.

Political institutions are themselves nested within political regimes. According to an influential definition, institutions "are the humanly devised constraints that shape human interaction" (North 1990: 3). Political institutions make up 'the rules of the game', proscribing who can engage in politics and how. First, they constrain behavior in the form of rules and regulations; they establish procedures to detect variations from the rules and regulations; and finally, they constrain the way in which the rules and regulations are specified and enforced through the establishment of moral, ethical, and behavioral norms. Thus, institutions can range from highly abstract notions, such as constitutional principles, to actual organizations, such as political parties, trade unions or the military. They include formal features, like the judiciary, as well as informal customs such as lobbying and patronage (Bratton and van de Walle 1997: 40). Here, I focus on a more formal aspect, namely restrictions on political parties. In particular, I distinguish between regimes that formally or de facto allow for none, single, and multiple parties.

1.1.3 Dictatorship

The term dictatorships has several connotations (see e.g. Gandhi 2008; Svolik 2012), and I am not going to enter any full-fledged discussion of its meaning here. Throughout the thesis, I will use the terms authoritarian regimes, autocracies and dictatorships interchangeably. In contemporary usage, the term is usually conceptualized as the opposite of democracy. Yet, even if we accept this criterion, defining dictatorship is by no means a straightforward exercise. First, there is disagreement as to whether democracy is a continuous or dichotomous variable (e.g. Adcock and Collier 2001; Munck and Verkuilen 2002). Second, there is disagreement as to whether democracy should be defined according to the existence of particular institutions (procedural definition) or according to some underlying principles (substantive definition) (Beetham 1999; Cheibub, Gandhi, and Vreeland 2010). Third, we also see disagreement on which particular traits that should be included in these definitions Knutsen (2011). As such, all approaches have their distinctive benefits and drawbacks, and no single index or categorization is withheld problems with reliability or validity (Cheibub et al. 2010; Munck and Verkuilen 2002).

Here, I concur with Adcock and Collier (1999) and Hadenius and Teorell (2005) that choosing between different democracy indices is first and foremost a matter of the research

question at hand, and should be guided by their theoretical and empirical underpinnings. To facilitate a particular focus on institutional variations within authoritarian regimes, I believe it is appropriate in my case to draw a discrete cutoff point between democracies and authoritarian regimes. Furthermore, there are compelling reasons for separating democracies from dictatorships on the basis of a substantive rather than a procedural definition. According to procedural conceptions of democracy, dictatorship is defined exclusively on the absence of certain institutions, typically competitive elections (e.g. Przeworski, Alvarez, Cheibub, and Limongi 2000; Schumpeter 2013). This approach may be problematic when studying the effects of political party restrictions, particularly in the case of dictatorships with multiple parties. Many multiparty autocracies hold elections that appear reasonably competitive and even occasionally result in alternations in office (Wahman, Teorell, and Hadenius 2013). However, if the elections are fraught with obstacles for the opposition in the period leading up to the balloting, it seems questionable whether voters can exercise a free choice. Thus, labeling these cases as democracies could seriously mask the electoral irregularities that take place in these regimes (Hadenius and Teorell 2007). In fact, so-called competitive authoritarian regimes have received significant scholarly attention in recent years, and often display dynamics that are highly divergent from most democracies (e.g. Diamond 2002; Levitsky and Way 2002; Schedler 2002). As such, excluding these cases from the list of dictatorships may potentially discard a lot of relevant information.

According to proponents of substantive definitions of democracy, institutions are necessary but not sufficient to characterize a regime as democratic. As Knutsen (2011: 57) contends, lists of institutional attributes "do not identify what democracy *is*, but rather identity crucial elements of what a functioning democracy *requires*". Hence, the embodiment of democracy does not reside in the presence of particular institutional traits, but instead the respect and pursuit of some core principles, most notably popular control over public affairs on the basis of political equality (Beetham 1999: 90; Dahl 1972). Popular participation in decision-making is a fundamental aspect in most public depictions of democracy, and its absence has been argued to be a source of some of the most renowned campaigns of nonviolent action, including the Anti-Apartheid Movement in South Africa and the People Power Movement in the Philippines (Nepstad 2011; Schock 2005).

In light of these considerations, I choose to draw the line between democracies and non-democracies on the basis of David Beetham's (1999) definition, and consider a country a dictatorship if it does not satisfy certain levels of political equality and popular control over decision-making. Basically, this implies that a country has to pass a determined threshold of

contestation *and* participation to qualify as a democracy; otherwise the regime is regarded as a dictatorship.[3] The operational details of where to place this threshold will be outlined in Chapter 4.

1.2 Plan of the Thesis

The thesis is organized as follows. In Chapter 2 I review the scholarly debate on political institutions and popular mobilization, and identify the key knowledge gap motivating this inquiry. In Chapter 3 I present the theoretical framework, and derive a set of empirically testable hypotheses relating to the effects of political party restrictions on nonviolent uprisings. Chapter 4 presents the quantitative research design, and the properties of the logistic regression model, which I use to test these hypotheses. The results of this analysis are reported in Chapter 5, along with robustness checks and measures of fit. Finally, some concluding remarks are offered in Chapter 6.

[3] By deciding a specific cutoff-point, the original definition is transformed from a continuous definition to a dichotomous one, which allows me to study dictatorships in isolation.

2 Literature review

> *No clapping is possible without two hands, no quarrel without two people, and no state without two entities: the rulers and the ruled.*
>
> Mahatma Gandhi.[4]

Research on nonviolent resistance campaigns has grown considerably in the last few years, particularly following the publication of Chenoweth and
's (2011) book on the subject (Celestino and Gleditsch 2013; Cunningham 2013; Shellman, Levey, and Young 2013; Sutton, Butcher, and Svensson 2014; Svensson and Lindgren 2011). As of yet, however, the literature on nonviolent campaigns is still in its infancy. It is dwarfed, for instance, by the literatures on revolutions (e.g. Goldstone 2001; Goodwin 2001; Huntington 1968; Kuran 1989; Skocpol 1979; Tilly 1993) and civil wars (e.g. Collier and Hoeffler 2004; Fearon and Laitin 2003; Gleditsch, Hegre, and Strand 2009; Hegre, Ellingsen, Gates, and Gleditsch 2001; Hegre and Sambanis 2006; Lai and Slater 2006; Peceny, Beer, and Sanchez-Terry 2002). Hence, important questions remain unanswered regarding the causes and dynamics of nonviolent campaigns (Chenoweth and Lewis 2013: 415).

In particular, we know relatively little about the initiation of nonviolent campaigns (Butcher and Svensson 2014; Chenoweth and Lewis 2013b; Dahl, Gates, Gleditsch, and González 2014). Yet, there do exist vast literatures on the emergence of social movements (e.g. McAdam, McCarthy, and Zald 1996; Meyer 2004; Tarrow 1998) and revolutions (e.g. Bueno de Mesquita and Smith 2010), and a growing literature on popular uprisings (e.g. Gandhi 2008; Kendall–Taylor and Frantz 2014; Svolik 2012). All of these concepts are closely related to nonviolent campaigns and deal with the broader concept of popular mobilization. This chapter will therefore first review the lessons from the literature on civil resistance, and then proceed with a review of the most relevant lessons from the literatures on social movements, revolutions and authoritarian survival. I will concentrate in particular on the findings that concern the effects of political institutions on popular uprisings. I argue here that the literature on authoritarian survival is especially relevant for investigating the association between political institutions and nonviolent campaigns in authoritarian regimes. Crucially, the literature attempts to nuance and disaggregate the concept of authoritarian regimes while also addressing the effects of political institutions on popular insurrections.

[4] Cited in Roberts and Garton Ash (2009: 375).

Based on insights from this literature, I identify the effects of restrictions on political parties as a particularly fruitful area for research.

2.1 Theories of Civil Resistance

In many ways, Gene Sharp's (1973) seminal work, *The Politics of Nonviolent Action*, laid the theoretical groundwork for modern accounts of nonviolent resistance with his theory of power. The premise of the theory is that the power of the government is fundamentally conditioned on the obedience and cooperation of the ruled. As Sharp (1973: 16) writes, "[the] most single quality of any government, without which it would not exist, must be obedience and submission of its subjects […] Obedience is the heart of political power". Thus, reminiscent of the words of Mahatma Gandhi stated in this chapter's preamble, the logic of nonviolent action is to deprive leaders of their main source of power: the acquiescence and obedience of the ruled. Once people actively remove their consent or cooperation, a regime's capacity to rule begins to crumble (Roberts and Ash 2009; Schock 2005; Sharp 1973).

As Schock (2013: 279-280) points out, a key insight from Sharp's study "is that violence is not required to topple powerful and repressive regimes. If a sufficient number of people refuse to obey or engage in actions that support the regime for a sufficient amount of time, then its power may be undermined or perhaps eliminated." Hence, regardless of a regime's brutality and coerciveness, active disobedience or refusal to cooperate will eventually make a regime unable to rule (Schock 2005: 37-38). Crucially, it is along these lines Chenoweth and Stephan (2011: 221) argue that nonviolent conflicts take place and succeed "against democracies and non-democracies, weak and powerful opponents, conciliatory and repressive regimes" and that "conditions shape – but do not predetermine – the capacity for a nonviolent resistance to adapt and gain advantage under even the direst of circumstances". Accordingly, the nonviolent resistance approach assumes that context ultimately cannot constrain the mobilization of nonviolent action. Instead, nonviolent action will often be most desirable in the most repressive and authoritarian circumstances (Chenoweth and Stephan 2011).

In general, the assumption that conditions do not dictate the outcomes of nonviolent campaigns have led civil resistance scholars to predominantly concentrate on explaining the *success* of nonviolent campaigns rather than their *onset*. In fact, until Chenoweth and Lewis (2013) published an article where they explicitly compared the onset of nonviolent and violent campaigns, it had commonly been assumed that the sources of nonviolent and violent

campaigns were broadly identical (Chenoweth and Stephan 2011: 32). According to Nepstad (2011: 16), "[reviewing] the literature reveals that the causes of violent and nonviolent revolutions are generally the same, but armed and unarmed struggles employ different strategies and have distinct dynamics". Studying the political conditions that precipitate nonviolent conflict has therefore received scant attention.

However, the limited attention to nonviolent campaign onset is not just the result of a lack of theoretical interest. Just as much it is a reflection of methodological decisions. As Schock notes,

> The predominant research method used in the study of civil resistance has been the case study of movements, campaigns, or streams of contention. Case studies, of course, are invaluable and may provide deep insight. Nevertheless, the field is characterized by selection bias in the sense that most studies have focused on single successful cases. We lack comparative studies that focus on multiple cases and compare and contrast successful and unsuccessful campaigns of civil resistance (2013: 268).

Thus, comparative assessments of nonviolent campaigns have traditionally been few. Indeed, among the comparative case studies that look at the emergence of nonviolent conflict, none – as far as I am aware - have systematically considered the role of political institutions (e.g. Ackerman and DuVall 2001; Nepstad 2011; Schock 2005; Stephan 2009).

Only recently have we started to see more systematic large-N studies on nonviolent campaigns. The publication of Chenoweth and Stephan's (2011) book has spurred a sharp increase in the number of quantitative studies of nonviolent resistance (e.g. Asal, Legault, Szekely, and Wilkenfeld 2013; Butcher and Svensson 2014; Celestino and Gleditsch 2013; Chenoweth and Lewis 2013b; Cunningham 2013; Kaplan 2013; Shaykhutdinov 2010; Shellman et al. 2013; Sutton et al. 2014; Svensson and Lindgren 2011). Like Chenoweth and Stephan these studies are concerned with comparing nonviolent and violent campaigns. They have focused on factors such as the effectiveness and success of nonviolent resistance compared to violent resistance (e.g. Svensson and Lindgren 2010; Shaykhutdinov 2010) and armed and unarmed movements' prospects for democratization (e.g. Celestino and Gleditsch 2013). Most importantly, some of these studies have also started to empirically map the onset of nonviolent campaigns (Asal et al. 2013; Butcher and Svensson 2014; Chenoweth and

Lewis 2013; Cunningham 2013; Dahl et al. 2014). Interestingly, they reveal a clear divergence in the determinants of nonviolent and violent campaign onsets.

The first quantitative study to systematically explore the determinants of nonviolent campaign onset was published by Chenoweth and Lewis (2013). First of all a preliminary study, the article was intended to illustrate how common predictors of civil war onset have very different coefficient estimates in the case of nonviolent campaigns. Replicating Fearon and Laitin's (2003) study of civil war onset, Chenoweth and Lewis find only one common determinant for the onset of unarmed and armed insurgencies: population size. Otherwise the remaining predictors for civil wars are either completely reversed or insignificant for nonviolent campaigns. The only significant predictors of nonviolent campaigns are flatter terrain and more authoritarian regimes. This last finding is important, however, as it confirms Chenoweth and Stephan's (2011: 66-67) original suggestion that nonviolent campaigns tend to emerge in more authoritarian settings than campaigns of violent resistance.

Cunningham (2013) performs a comparable investigation into the determinants of self-determination disputes. Analogous to Chenoweth and Stephan (2013) she finds that nonviolent campaigns, as compared to conventional politics, are more likely to occur in less democratic contexts. This finding she attributes to the existence of institutional channels. As she writes, "[democracies] generally provide greater opportunity for aggrieved groups to seek redress through conventional political channels. As such, democracies are more likely to be more responsive to citizen demands and grievances than non-democracies" (Cunningham 2013: 295).

Similarly, Butcher and Svensson (2014) also find a negative association between the level of democracy and nonviolent campaign onset, further corroborating Chenoweth and Stephan's (2011) original finding. Like Cunningham, they suggest that democracies are less likely to experience nonviolent conflict because they channel grievances through formal institutions. However, they also show that anocracies - the class of intermediate regimes between consistent autocracies and democracies – have the highest likelihood of experiencing nonviolent conflict. This finding, they suggest, may be due to either of the following: (1) anocracies make mobilization easier while reducing the threat of severe repression, (2) anocracies allow for some popular mobilization without meeting societal demands for change, or (3) the finding is an artifact of middle-range regimes being contaminated by a measure of political violence in the Polity IV index (Butcher and Svensson 2014: 16). This third suggestion is important. It points to a set of criticisms against the Polity scale that are

relevant for all of the previous findings on the association between autocratic regimes and nonviolent uprisings.

Crucially, all of the aforementioned publications use the Polity scale for measuring regime attributes. The Polity index is a combined index of the level of autocracy and democracy in a country, ranging from -10 to 10, where 10 is the most democratic. Although frequently used in political science in general, the index has been increasingly questioned as appropriate for studying political conflict (Fjelde 2010). First, as indicated above, the Polity index has been considered inappropriate because the measure includes endogenous components related to political violence. As such, anocracies are partly defined by the presence of political violence (Vreeland 2008). Second, questions have been raised as to whether a single scale is suitable for identifying regime characteristics. According to Fjelde (2010: 198), "[t]he use of a one-dimensional and aggregate index [...] neglects qualitative differences between regime types", as "the additive nature of the Polity scale implies that very different configurations of authority structures can underlie the same Polity score". Thus, the Polity index is more concerned with the level of 'democraticness' in a country than variations in institutional configuration, thereby effectively reducing authoritarian regimes to a residual category (Gleditsch and Ward 1997). Thus, if we are to explain why nonviolent campaigns are more likely in authoritarian regimes, we obviously have to take into account the institutional heterogeneity among authoritarian regimes. I return to this point later.

Another problem is the fact that none of the studies mentioned above feature political institutions or regime attributes as their main point of interest. Consequently, these authors have made few attempts to elaborate the association between authoritarian regimes and nonviolent campaign onsets. As of yet, no theoretical account on the effect of political institutions on nonviolent campaigns is available. In part, this is because nonviolent resistance scholars have typically turned to other theoretical traditions for insights on the emergence of mass uprisings. In particular, the onset of mass uprisings has historically been the domain of social movements and revolutions scholars.

2.2 Theories of Social Movements and Revolutions

As noted by Shock (2013: 280), scholars of social movements and revolution study several of the same phenomena as civil resistance scholars, but there has generally been a lack of engagement between the respective research traditions. Social movements scholars have for

instance been particularly concerned with the mobilization of protests, which except for riots are predominantly peaceful. Similarly, apart from the fact that revolutions can also be violent, modern conceptions of revolutions correspond closely to the understanding of nonviolent campaigns outlined in the previous chapter (see Goldstone 2001 for review).[5] Thus, these literatures are worth closer inspection, particularly in light of their greater attention to contextual variables than the civil resistance literature (Schock 2013).

One of the focal differences between civil resistance research and research on social movements and revolutions is the relative emphasis on the role of agency versus structure. As noted previously, scholars within the civil resistance literature have typically highlighted the role of agency, emphasizing how a people's collective and active removal of consent eventually makes a ruler unable to rule (e.g. Sharp 1973). In contrast, scholars of social movements and revolution have tended to provide more structure-oriented explanations of mass mobilization. Theories of revolution have for instance devoted considerable attention to the role of economic, political, and demographic change in altering class relations and state structures to produce revolutions (e.g. Goldstone 2001; Skocpol 1979). Similarly, theories of social movements have emphasized the importance of mobilizing structures and the political context in determining the mobilization of social movements (e.g. McAdam, McCarthy, and Zald 1996; Meyer 2004).[6] These accounts of mobilization thus differ markedly from the more bottom-up explanations provided by scholars of nonviolent resistance.

In particular, political opportunity approaches are prevalent within both research on social movements and revolutions. Political opportunity approaches argue that the mobilization and success of movements depend on the opening and closing of opportunities created by the structure of the political order. They explicitly address the effects of contextual variables on popular mobilization. As Meyer (2004: 126) contends, "[t]he key recognition in the political opportunity perspective is that activists' prospects for advancing particular claims, mobilizing supporters, and affecting influence are context-dependent". Political opportunity structures are thus "consistent dimension of the political environment which either encourage or discourage people from using collective action" (Tarrow 1998: 18).

'Political opportunity structures' is a ubiquitous concept though. It is given all sorts of meanings in individual case studies in the literatures on social movements and revolutions,

[5] Goldstone's (2001: 142), for instance, define revolutions as "effort[s] to transform the political institutions and the justifications for political authority in a society, accompanied

[6] According to Schock (2005: 28), "mobilizing structures are the networks through which the mobilization of tangible resources, people, and organized collective action occurs."

and captures both formal and informal features of the political context (McAdam et al. 1996: 23-31; Schock 2005; Tarrow 1998: 19-20).[7] Still, four variables in particular are commonly associated with political opportunities: (a) increasing popular access to the political system, (b) divisions within the elite, (c) the availability of elite allies, and (d) the state's capacity and propensity for repression (Goodwin 2001; McAdam et al. 1996; Meyer 2004; Schock 2005; Tarrow 1998). Crucially, several of these dimensions – most importantly the extent of political openness - can be considered in light of the institutional structure of the regime.

2.2.1 Political Institutions and the Mobilization of Social Movements

Within the literature on social movements several relationships have been suggested regarding political opportunities and protests in democracies. Peter Eisinger (1973) (1973) and Charles Tilly (1978), for instance, posited a curvilinear relationship: neither full access nor its absence produce the highest frequency of protests. According to Tilly, extensive political openness discourages protests because less costly, more direct routes of influence are available. Conversely, more repressive authorities make groups unable to develop the requisite capacity to mobilize protests. Others have suggested that the opening or constriction of opportunities provoke extrainstitutional mobilization (e.g. McAdam 1999; Meyer 2004; Tarrow 1989). The problem with these theories is that they are mainly tailored to challenges in developed democracies (Meyer 2004: 132; Schock 2013: 280). Consequently, there has traditionally been limited attention to whether these insights are applicable to authoritarian regimes.

That said, in recent years, there has been an increasing interest in applying social movements theory also to challenges in non-democratic and democratizing regimes (Adler and Webster 1995; Bermeo 1997; Bratton and van de Walle 1997; Collier and Mahoney 1997; Goodwin 2001; Schock 2005; Tarrow 1998). For instance, based on a macro-analysis of one hundred political challenges from 1786 to 1996, Jeff Goodwin (2001) finds that political opportunities are probably even more relevant for the emergence of social movements and revolutions in authoritarian regimes than in democracies.[8] Also Schock (2005: 29) suggests that "political opportunities may be even more important for challenges in nondemocracies than in democracies, since opportunities for dissent are less common and

[7] Sidney Tarrow (1998: 19-20), for instance, defines political opportunities as "consistent – but not necessarily formal or permanent – dimensions of the political struggle that encourage people to engage in contentious politics".

[8] A similar conclusion is reached in a later publication (Goodwin 2011).

therefore any signs of political opportunity are likely to generate opposition". Similarly, Tarrow (1998: 77-78) argues that political protests may be provoked by the expansion of popular access in authoritarian regimes, especially if a regime agrees to hold elections.

Yet, apart from some preliminary tests by Goodwin (2001, 2011), systematic large-N studies of the effects of political opportunities on protests have been few (Goodwin 2011; Meyer 2004). Consequently, the effects of political institutions on protests in non-democratic regimes has received scant attention in the literature on social movements (Bratton and van de Walle 1997). Partly this is because most analyses of social movements have been conducted through case studies. Little research has systematically examined the impact of contextual variables on protests in general (Goodwin 2011; Schock 2013). Moreover, the case studies that focus on the association between political openness and political protests (e.g. Eisinger 1973; McAdam 1999; Meyer 2004; Tarrow 1989, 1998; Tilly 1978), typically do not operationalize access to the political system in terms of formal institutional features per se, but rather in terms of *de facto* access to the political system (Meyer 2004). Recent studies on social movement mobilization in authoritarian contexts, like the ones by Tarrow (1998) and Goodwin (2001), have also focused primarily on the contraction or expansion of political opportunities and political access rather than more formal features of the political system like restrictions on political parties or the form of leadership selection.

One notable exception is Bratton and van de Walle's (1997) study of democratization in Sub-Saharan Africa. Employing a politico-institutional approach, the authors study the effects of institutional pluralism and elections on political protests in 47 Sub-Saharan African countries in the decade between 1985 and 1994. Through quantitative assessments they find that political protests were most likely to erupt in single party regimes, followed by multiparty regimes and regimes that banned political parties, which mostly consisted of military oligarchies. Bratton and van de Walle also found that the timing of protests was significantly related to the degree of historical experience with political competition.[9] As they observe, "[t]he longer a country's institutional experience with competitive party systems, the sooner that prodemocracy protest was likely to break out" (Bratton and van de Walle 1997: 146).

As far as I know, no comparable study of political protests has been done featuring a global sample. Bratton and van de Walle's (1997) book was among the first studies to systematically test theoretical arguments about different kinds of dictatorships in general.

[9] Political competition was here operationalized in the form of an index recording the years under multiparty, one-party, and no-party rule.

However, their work has helped inspire a burgeoning of quantitative research on the effects of dictatorial types and institutions (e.g. Gandhi 2008; Geddes 1999), democratization (e.g. Hadenius and Teorell 2007) and violent conflict (e.g. Peceny, Beer, and Sanchez-Terry 2002). I address this literature in section 2.3.

2.2.2 Political Institutions and Revolutions

Unlike theories of social movements, research on revolutions has historically tended to focus on contention in developing countries (e.g. Skocpol 1979). Particularly contextual explanations that emphasize the relationships among state authorities, elites, and popular groups have garnered attention in the field. For instance, several case studies highlight the resilience of fiscally and militarily sound states with united elites to revolution from below (Goldstone 2001: 146; Goodwin and Skocpol 1989: 497). Political opportunities like newly called elections (e.g. Bunce and Wolchik 2006) and succession crisis following the retirement of a long-standing ruler (e.g. Hale 2006) are also argued to heighten the likelihood of revolution. Importantly, the attributes of authoritarian regimes have received significant attention. Several case studies highlight the vulnerability of exclusionary regimes like colonial regimes and personalist regimes (Goldstone 2001; Goodwin and Skocpol 1989; Snyder 1998). Others emphasize the relative immunity of democracies and inclusionary authoritarian regimes to revolutionary transformations (Goodwin and Skocpol 1989: 495-496). According to Goodwin and Skocpol (1989: 495),

> Revolutionary movements, history suggests, typically coalesce in opposition to closed or exclusionary, as well as organizationally weak (or suddenly weakened), authoritarian regimes. By contrast, multiparty democracies or quasi democracies […] have not facilitated the growth of revolutionary coalitions. The ballot box […] has proven to be the coffin of revolutionary movements.

Again, the lack of access to the political system is highlighted as an important driver of mobilization. Yet, despite considerable attention to contextual variables in the literature on revolutions, there have been few attempts to systematically test these intuitions. Partly this is because there have been relatively few quantitative studies on revolutions in general (Knutsen 2014: 495; Schock 2013).[10] A common contention is that revolutions are inherently

[10] One notable exception is Bueno de Mesquita and Smith's (2010) study of government finance and revolutions. Investigating the relationship between the level of nontax revenue and revolutionary attempts, the

difficult to predict (Kuran 1989). Also recent work on revolutions within the literature on contentious politics have been primarily conducted through case studies (Schock 2013).

Studies that have addressed quantitatively the effects of regime type tend to focus on the onset of violent revolutionary activities rather than nonviolent uprisings (e.g. Collier and Hoeffler 2004; Fearon and Laitin 2003). Typically, these studies have focused on Hegre et al. (2001) and Fearon and Laitin's (2003) original finding that semidemocracies are more prone to civil war (Gleditsch et al. 2009; Goldstone et al. 2010), findings that later have been shown to be driven largely by the coding of the Polity scale (Vreeland 2008).[11] Two interesting exceptions are the studies by Lai and Slater (2006) and Fjelde (2010). Applying categorical measures of regime type, both articles find that military regimes are particularly prone to political violence, while single-party regimes seem to be exceptionally resilient to armed challenges to their authority. Both of these studies build directly on insights from the literature on autocratic survival. Crucially, I also believe that this literature can help shed some new light on the association between political institutions and nonviolent uprisings.

To summarize, the literatures on social movements and revolutions deal quite extensively with the effects of contextual variables on the initiation of popular mobilization, and offer considerable attention to the importance of political opportunities. However, despite the fact that several of these accounts also touch the effects of political institutions, they tend to be more generalized. However, political access appears to be a recurring theme. Yet, apart from in studies of violent rebellions, its role has not been systematically addressed in quantitative studies.

2.3 Theories of Autocratic Survival

Inspired by theories on revolutions and democratization, a growing literature has evolved that attempts to explain and assess the determinants of regime stability and democratization in authoritarian regimes. I refer to this literature as the literature on autocratic survival. What sets this research apart from previous studies on the same phenomena is the desire to empirically map the outcomes of different kinds of authoritarian regimes. After Barbara

authors find few significant determinants of revolutionary attempts apart from the number of revolutionary attempts in the previous year. They also test the association between the size of a regime's winning coalition – the number of supporters the leader needs to stay in power - and revolutionary attempts, but find no significant relationship.

[11] Other authors have challenged these findings on the grounds that the Polity scale incorporates factionalism in the midrange of its two participation components (Cheibub et al. 2010; Gleditsch et al. 2010; Goldstone et al. 2010; Vreeland 2008).

Geddes (1999) provided the first comprehensive empirical study of different types of dictatorships on a global scale, several quantitative studies of authoritarian regimes have followed suit (e.g. Gandhi 2008; Gandhi and Przeworski 2006; Geddes, Frantz, and Wright 2014; Hadenius and Teorell 2007; Magaloni 2008; Magaloni and Kricheli 2010; Svolik 2012; Wahman, Teorell, and Hadenius 2013). Until then quantitative studies of political regimes had mostly lumped autocracies into one category, contrasting them to democracies either through dichotomous or continuous measures. Consequently, dictatorships have often been left as a residual category, without considering the heterogeneity they display in terms of institutional configuration. As Geddes (1999: 121) emphatically makes clear, "different kinds of authoritarianism differ from each other as much as they differ from democracy". Autocracies have been shown to experience great variation in terms of outcomes such as regime durability (e.g. Gandhi 2008; Geddes 1999; Magaloni 2008; Svolik 2012), democratization (e.g. Hadenius and Teorell 2007; Magaloni and Kricheli 2010) armed conflict (e.g. Peceny et al. 2002; Weeks 2012), economic growth (e.g. Wright 2008), and property rights protection (e.g. Knutsen and Fjelde 2013).

As a result of these endeavors, multiple regime typologies are now available as alternatives to the Polity scale. The three most frequently used measures are the classifications offered by Geddes et al. (2014), Cheibub et al. (2010), and Wahman et al. (2013) (hereafter GWF, CVG, and HTW). On the one hand, these subdivisions differ appreciably in terms of theoretical focus and coding criteria. For instance, while GWF and CVG classify dictatorships according to the social origins or characteristics of the ruling elites, HTW base their classification on the institutional attributes of authoritarian regimes. Moreover, GWF stand out given that they designate personalistic regimes as a separate regime category. On the other hand, the three subdivisions also share some distinct commonalities. All three classifications include both military regimes and monarchies. Moreover, both GWF and HTW give significant attention to party regimes, with GWF designating a separate category for dominant party regimes, and HTW separating between one-party and multiparty regimes. Given the frequent use of these three classifications, it is also evident that much theoretical attention has been geared towards these particular features of authoritarian regimes.

Crucially, popular uprisings have also been one of the focal points of theoretical interest. In fact, until quite recently, the conventional understanding in the literature has been that mass uprisings are the main threat to a dictator's rule (e.g. Acemoglu and Robinson 2001; Boix 2003; Gandhi 2008; Gandhi and Przeworski 2006). These events correspond closely to

what I have referred to as nonviolent campaigns. Like nonviolent campaigns they are 'popular' in the sense that they are civilian-based and carried out through widespread political participation (Schock 2005; Svolik 2012: 3-5). They are also maximalist in the sense that they aim to overthrow or significantly change an established authority. As such, they are often more sustained than mere protests or riots (Geddes 1999; Geddes et al. 2014).[12] Typically popular uprisings also include the use of nonviolent tactics (Chenoweth and Ulfelder 2015),[13] and unlike revolutions, uprisings do not include armed confrontations (Geddes et al. 2014: 317). Thus, the term popular uprising arguably overlaps quite closely to the definition of nonviolent campaigns introduced in Chapter 1, and perhaps even closer than the broader terms social movements, protests and revolutions. It is therefore surprising that this literature has received such limited attention from civil resistance scholars (Chenoweth 2015).

2.3.1 Political Parties and Popular Uprisings

All dictators face threats from the masses, and thus carefully have to balance against the majority excluded from power. As dictators by definition hold power without popular consent, they become vulnerable to popular challenges to their rule (Bueno de Mesquita, Smith, Siverson, and Morrow 2003; Svolik 2012). Yet, authoritarian regimes vary greatly in terms of the relative threat to their rule. While some face strong and pressing opposition, others seem largely invulnerable to popular challenges. To explain these differences, scholars of authoritarian politics have highlighted two central mechanisms: repression and co-optation (Fjelde 2010; Svolik 2012). Repression is pervasive in many authoritarian regimes, and has repeatedly been identified as one of the key ways that dictators preclude regime change (e.g. Acemoglu and Robinson 2001; Boix 2003; Wintrobe 1998). Yet, autocrats do not remain in power solely through the use of coercion. Positive incentives are also argued to play a key role in soliciting cooperation. In return for compliance, the dictator promises potential opponents and the opposition offers of spoils such as power positions, rents or policy concessions (Gandhi and Przeworski 2006).

A central claim in the literature on authoritarian survival is that the success of both coercion and co-optation strategies hinge on the institutional configuration of the

[12] Although scholars of authoritarian politics have mainly been concerned with uprisings against governments or political leaders, these campaigns could also pursue territorial goals like increased autonomy or secession, such as in the former USSR and Yugoslavia.

[13] While they may also include the use of violent tactics, armed confrontations with government authority are more commonly referred to as insurgencies (Geddes et al. 2014: 317).

authoritarian regime (e.g. Boix and Svolik 2013; Fjelde 2010; Geddes 1999; Hadenius and Teorell 2007; Magaloni 2008; Magaloni and Kricheli 2010). In particular, the literature has devoted considerable attention to the role of political parties in precluding popular mobilization (e.g. Gandhi 2008; Geddes 1999; Magaloni 2010; Svolik 2012). Political parties are the most widespread instrument of authoritarian control in dictatorships, and have been singled out as especially effective at coopting both present and potential opposition (see Magaloni and Kricheli 2010 for review). Single and dominant party regimes also tend to maintain vast repressive apparatuses, and have been very successful in subordinating the military to political control (Peceny et al. 2002; Svolik 2012). In general, the effect of political parties has received significant attention in empirical studies of authoritarian survival (see also Boix and Svolik 2013; Wright and Escribà-Folch 2012), and has also been noted in case studies of nonviolent resistance (e.g. Nepstad 2011; Schock 2005). Hence, this topic seems like a fruitful starting point for exploring the institutional determinants of nonviolent campaigns.

Although political parties are widely conceived as important contributors to the survival of numerous dictatorships, there is nevertheless some disagreement as to exactly why political parties are such apt instruments of authoritarian control. In particular, much debate has centered on whether allowing a single party or allowing multiple parties provides the most effective means of co-optation. In large part, this contention stems from different opinions on who constitute the primary targets of party co-optation. To be specific, scholars typically specify different targets of co-optation depending on whether their theoretical focus is on the regime party or opposition parties. Regime-sanctioned parties have been argued to be significant in retaining the support of ruling elites (Magaloni 2008; Magaloni and Kricheli 2010) and co-opting ideologically close segments of the population (Svolik 2012), while opposition parties have been noted as crucial in co-opting the external opposition, i.e. the population at large (Gandhi 2008; Gandhi and Przeworski 2006). Depending on who are identified as the main targets of co-optation, we arrive at different suggestions regarding whether permitting one or several parties constitutes the best instrument of co-optation.

If the primary target of co-optation is the ruling elite and ideologically proximate segments of the population, then dictatorships with single-party dictatorships are arguably superior in co-opting potential dissidents. Several authors have highlighted the relative immunity of dominant and single-party autocracies to internal splits (Boix and Svolik 2013; Geddes 1999; Magaloni 2008; Magaloni and Kricheli 2010; Svolik 2012). Regime parties are instrumental in enforcing power-sharing deals that prevent defections from both the dictator

and his allies. Regime parties entail both regular interaction and formal rules of interaction that increase transparency and facilitate the detection of the dictator's noncompliance (Svolik 2012). By controlling succession and access-to-power positions, party regimes give party cadres and ideologically proximate segments of the population the incentive to invest in the regime under the anticipation that they will eventually be promoted into rent-paying-positions (Magaloni and Kricheli 2010; Svolik 2012). However, as Magaloni (2008) suggests, multiparty elections may undermine the credibility of these power-sharing deals by heightening the risk of an electoral split. Elite cohesion is thus more fragile in multiparty autocracies, as the dictator has to distribute more spoils to prevent defections. Consequently, the value of the private rents accrued to each party member decreases and therefore also the pay-offs from investing in the current regime (Bueno de Mesquita et al. 2003; Fjelde 2010).

However, if the primary target of co-optation is the general opposition, then multiparty legislatures and elections provide important venues for dissent that are not available in single-party regimes. Gandhi and Przeworski (2006) and Gandhi (2008), for instance, expect multiparty autocracies to be better at co-opting the opposition because they provide more effective forums for dealing policy concessions than countries with single-party legislatures. By allowing opposition parties, multiparty legislatures formalize the representation of regime opponents and reduce uncertainty over whether the opposition will be included in future bargaining with the regime. Similarly, Geddes has highlighted the role of opposition parties in co-opting the general opposition:

> Single-party regimes survive in part because their institutional structures make it relatively easy for them to allow greater participation and popular influence without giving up their dominant role in the political system. Most single-party governments have legalized opposition parties and increased the space for political contestation. (1999: 135)

This suggestion is congenial to Schedler's (2002) argument that multiparty elections encourage both rulers and the opposition to invest in these institutions. As the author writes, "once elections cross a hard-to-specify but real threshold of openness and competitiveness […] elections stop being shams and start playing 'enough of a role in the constitution of power' to compel both rulers and opposition forces 'genuinely to care' about them" (Schedler 2002: 38). They become "constitutive of the political game" (Schedler 2009: 387). By contrast, regimes that only allow one party do not encourage the opposition to invest in

political institutions to the same extent. Although single-party regimes are more inclusive and provide greater access to the political arena than regimes that ban political parties, they still do not institutionalize access like multiparty autocracies (Gandhi 2008).

The key point to realize here is that these accounts are not necessarily as incompatible as some of the authors suggest (Magaloni 2008). Regime parties and multiparty competition may both be significant co-optation strategies, but directed at different audiences. Single-party and dominant party regimes have been shown to be less vulnerable to elite defections and coup d'états than for instance competitive authoritarian regimes and military regimes (Geddes 1999; Svolik 2012). Yet, this does not necessarily make them more apt at soliciting the cooperation of the population at large. Bueno de Mesquita and his colleagues (2003, 2010), for instance, suggest that regimes with broader ruling coalitions – typically multiparty regimes – are more susceptible to internal splits, but less prone to revolutionary attempts than regimes with smaller ruling coalitions (e.g. single-party regimes, monarchies and military regimes). Correspondingly, the literature reviewed above also suggests that the same mechanisms that make multiparty autocracies more prone to internal splits than single-party autocracies actually make them *less* likely to incite popular uprisings. Magaloni's comment on the role of multiparty elections is telling:

> This course of action is not available in single-party regimes, where potential rivals can only attempt to challenge the dictator through violent means (e.g. by seizing power through a military coup, mounting a costly rebellion, or risking repression by organizing a subversive coalition or mobilizing one's followers into the streets). Thus, where multiparty competition is allowed, potential rivals possess two alternative courses of action: engage in violence or compete for power through elections (this second option is less costly). (2008: 13)

Thus, although multiparty competition heightens the risk of elite defections, it also provides the opposition and potential rivals with an alternative venue for mobilization that is far less costly than extralegal action. As Geddes et al. (2014: 327) contend, "Popular uprisings may be one of the few opposition strategies available […] where autocratic regimes have prevented the development of mass opposition party networks". Hence, it can be argued that multiparty elections encourage both potential opponents and the opposition to challenge rulers through conventional channels rather than through a nonviolent uprising.

2.3.2 Empirical contributions

As the label suggests, quantitative studies of authoritarian survival have predominantly focused on the survival of authoritarian regimes rather than their exposure to potentially regime-destabilizing threats. As such, regime change has been analyzed primarily through the use of quantitative techniques such as survival analyses or Markov chains (Gandhi 2008; Geddes 1999; Hadenius and Teorell 2007; Magaloni 2008; Magaloni and Kricheli 2010), which model the relative length of different regime types and their likelihood of regime transitions. The unit of analysis has therefore typically been the length of the tenure of authoritarian rulers, ruling coalitions or regimes. In terms of their findings, a majority of these studies have highlighted the resilience of single-party regimes and monarchies, as well as the relative instability of military regimes and multiparty autocracies (Hadenius and Teorell 2007; Magaloni 2008; Magaloni and Kricheli 2010).

Crucially, one cannot infer directly from these studies whether single-party regimes are less likely to experience popular uprisings. An important point made by more recent contributions within the literature is that popular uprisings have not – as has commonly been suggested - been the most frequent source of nonconstitutional leader exit in authoritarian regimes. Instead, the by far most prominent method of ousting leaders in the 20th century was coup d'états, i.e. the forced removal of authoritarian leaders by regime insiders (Goemans et al. 2009; Kendall–Taylor and Frantz 2014; Svolik 2012).[14] By Svolik's (2012: 5) estimates, only 11 percent of the nonconstitutional leader exits in authoritarian regimes between 1946-2008 were due to popular uprisings. The comparative figure for coup d'états is 68 percent. Certainly, popular uprisings have become far more common the last few decades, and have been responsible for the majority of authoritarian leader exits the last two decades (Kendall–Taylor and Frantz 2014). Nevertheless, the general implication is that the majority of the extant literature has not analyzed popular uprisings per se, and that we may find different results if we limit the analysis to nonviolent uprisings.

Still, two studies stand out, and merit particular attention. The first study was conducted by Jennifer Gandhi (2008), and investigates the impact of the number of legislative parties on the length of chief executives' tenures (see also Gandhi and Przeworski 2006).[15] By right-censoring deaths in office and controlling for coup d'états and civil wars, her analysis

[14] Note that Svolik's (2012) definition of coup d'états includes both removals by the military *and* other regime insiders.

[15] Her explanatory variable, Institutions, is an ordinal variable counting the number of legislative parties. The variable is coded 2 if the dictatorship has a legislature with multiple political parties, 1 if all legislative seats are held by the regime party, and 0 if either parties are banned or the legislature is closed.

provides a closer approximation to the effects of political parties on popular uprisings than some of the studies mentioned above. Importantly, her results reveal no significant relationship between the number of legislative parties and the length of tenure in office. However, according to Gandhi this does not constitute a null finding. Instead, it should be interpreted as a sign that political parties do have an effect on political survival, because dictators are able to respond with the appropriate levels of institutionalization to neutralize dissent. Thus, although regimes with higher levels of institutionalization face stronger opposition movements and are more dependent on cooperation, they still face no higher risk of rebellion.

In another influential study, Milan Svolik (2012) investigates the impact of legislatures and political parties on authoritarian stability. Unlike the majority of previous accounts he also studies the impact of political parties on coups and uprisings separately. In the case of uprisings he finds a significant, albeit weak, association between the presence of political parties and the length of authoritarian ruling coalitions.[16] Crucially, he attributes these findings to the ability of party regimes to institutionalize power-sharing, which he argues make them more capable of withstanding challenges from the masses. In particular, he highlights the role of strong regime parties in facilitating power-sharing and authoritarian control, and identifies single-party and dominant party regimes as the most durable regimes overall.

Despite arriving at different conclusions, both authors suggest that political parties play instrumental roles in reducing the threats of popular rebellions. However, none of the studies reviewed above investigate whether party regimes are less exposed to popular uprisings in general. Although the onset of nonviolent uprisings is certainty different from the success of nonviolent uprisings, several of these insights should carry relevance for the mobilization of nonviolent movements, particularly those movements that aim for regime change. Gandhi's account, for instance, in theory encompasses both successful and unsuccessful campaigns (2008: 143). Judging by the literature reviewed above, I also argue that her account holds particular promise for explaining the mobilization of nonviolent uprisings. Yet, based on the insights from Svolik (2012) and others, it seems doubtful whether we can expect such a uniform effect of the level of institutionalization on the risk of nonviolent uprisings. Moreover, none of the authors consider how regime duration plays into this calculation.

[16] Note that Boix and Svolik (2013) also briefly assess the impact of having political parties on the likelihood of popular revolts. Mainly their study is concerned with coup d'états. Their findings are nonetheless congenial to those in Svolik (2012).

Thus, in the next chapter I present a theoretical framework that attempts to combine these lessons with theoretical and empirical insights on nonviolent campaigns.

To summarize, the literature on authoritarian survival provides more detailed discussions of the role of political institutions in authoritarian regimes than comparable discussions in the literatures on social movements, revolutions, and civil resistance. Thus, although the literature on authoritarian survival highlights some of the same factors as the literatures on social movements and revolutions - including the role of political access - the literature provides more specific explanations of the role of particular institutional traits. Furthermore, some of these lessons appear to carry particular relevance for explaining nonviolent campaigns, especially writings on political parties and co-optation.

2.4 Summary

In this chapter I have reviewed the scholarly debate on political institutions and political mobilization, focusing in particular on the initiation of mass uprisings in authoritarian regimes. Following an inspection of the most relevant insights from the literatures on civil resistance, social movements, and revolutions, I identified the literature on authoritarian survival as a particularly fruitful reference point for studying civil resistance in authoritarian regimes. Specifically, I argued that some of the literature's lessons on the effects of restrictions on political parties should carry direct relevance to the onset of nonviolent campaigns. These lessons relate particularly to the role of party co-optation, which is argued to be instrumental in channeling popular dissent. The next chapter will present a theoretical framework that builds directly on these insights.

In conclusion, I want to stress the relevance of studying the determinants of nonviolent campaigns for scholars writing about social movements, revolutions, and authoritarian survival. As suggested here, these literatures have not only studied much of the same phenomena, but have also traced similar patterns and produced comparable insights. Hence, studying the mobilization of civil resistance can deepen our understanding of social movements, revolutions and authoritarian survival as well.

3 Theory

> *Dictators are not in the business of allowing elections that could remove them from their thrones.*
>
> Gene Sharp (2012: 6).

This chapter presents a theoretical framework for how political institutions affect the mobilization of nonviolent uprisings. In particular, I wish to address the effect of political parties on the propensity of civilians to engage in nonviolent uprisings in authoritarian regimes. As argued in the previous chapter, political parties and party regimes have been given extensive attention in the literature on authoritarian survival, but merit more attention in the case of nonviolent campaigns. Therefore, I opt for an integrative approach that draws on both research traditions to explain the association between autocratic regimes and nonviolent uprisings.

The general argument presented here is that the level of restrictions on political parties serves as a key determinant of whether the opposition chooses to pursue regime change through institutional channels, as opposed to nonviolent direct action. However, this 'channeling effect' is neither linear across different levels of institutionalization, nor constant over time. Dictatorships face varying levels of opposition and differ in their ability to co-opt opposition. Moreover, dictatorships experience different patterns over time, depending on the type of institutions that are established. Hence, authoritarian regimes should display predictable differences in their ability to prevent nonviolent uprisings.

The chapter will proceed as follows. I start off by presenting the basic premises and theoretical backdrop of the theory. Next, I present two models: a general model and a conditional model. In the first model authoritarian rulers face a significant dilemma when confronted with a stronger opposition. Although permitting more political parties can be important in facilitating co-optation, allowing more than one party is also associated with a greater risk of internal defections. Thus, single-party regimes should be particularly reluctant to reform, and will tend to underinstitutionalize in the face of a stronger opposition. The second model provides an extension of the general model, and posits that the effects of political party restrictions are conditioned on the duration of a regime. It suggests that the stabilizing properties of allowing multiple parties should decline over time. The main implications of the two models are (1) single-party regimes face a higher risk of nonviolent

uprisings than both no-party and multiparty autocracies, and (2) the impact of regime duration should be higher in multiparty autocracies than in single-party and no-party regimes.

3.1 Refinements to Civil Resistance Theory

The key notion of Sharp's (1973) theory of power – and much subsequent work within the literature on nonviolent resistance - is that active disobedience and refusal to cooperate will eventually make the regime unable to rule. No government can rule without the obedience and submission of its subjects (see also Chenoweth and Stephan 2011; Schock 2005, 2013). Though striking, scholars on authoritarian politics suggest that this assertion is overly simplistic. First, it ignores the extent to which regimes are dependent on cooperation and the relative strength of the opposition (Gandhi and Przeworski 2006). Second, regimes may vary in their ability to prevent mobilization and ensure cooperation (e.g. Svolik 2012). Several scholars suggest that these factors are intrinsically related (e.g. Bueno de Mesquita and Smith 2010; Pepinsky 2014). The stronger the opposition, the more likely is the regime to respond with new measures to curtail its strength. Hence, to understand where and when the opposition is likely to mount a rebellion we have to factor in the effects of both regime capacity and opposition strength (Gandhi 2008).

In general, the problem is not that civil resistance scholars do not recognize these aspects, but rather that they are largely left unaddressed. The importance of *leverage* is duly noted in the civil resistance literature (Schock 2013: 283). Leverage refers to "the capacity of a challenge to sever the opponents from the sources of power upon which it depends, either directly or through allies or third parties" (Schock 2013: 283). Chenoweth and Stephan (2011: 45, 221), for instance, admit that it may be harder for civil resistance campaigns to achieve leverage vis-à-vis some regimes than others, noting the resilience of rentier states to domestic pressures.[17] However, because civil resistance scholars are mainly concerned with the success of nonviolent movements, discussion has mostly revolved around how movements can take advantage of political, economic and moral dependence relations between the oppressors and the suppressed, rather than where movements can have more or less influence (Schock 2013). As such, it has been more important to show that revolutions *can* occur anywhere, rather than studying the extent to which they *do* occur anywhere. Partly,

[17] By rentier states, they refer to regimes that "rely on external sources, including export sales in natural resources, tourism, and economic aid for a sizable portion of net income" (Chenoweth and Stephan 2011: 45).

the focus on campaign success is also due to the literature's privilege of agency over contextual factors (Chenoweth and Ulfelder 2015: 3).

In particular, acknowledging that different regimes are more or less dependent on the opposition is crucial if we are to study the effects of political institutions on nonviolent uprisings. Otherwise, we risk committing serious methodological errors. For one, we ignore a potential endogeneity issue: the initial choice of political institutions in a country may be dependent on the strength of the opposition and the need for cooperation. In fact, this is a common claim within the literature on authoritarian politics. For instance, Gandhi and Przeworski's (2006) main argument is that the level of institutionalization and the number of legislative parties in dictatorships is a direct response to the strength of the opposition and the need for cooperation. Similarly, Svolik (2012: 10-11) claims that military dictatorships tend to arise in circumstances when it is critical to suppress the opposition. As he notes,

> [M]any dictators do not have much leeway when deciding how much to rely on soldiers for repression. In regimes that face mass, organized, and potentially violent opposition, the military is the only force capable of defeating such threats (Svolik 2012: 10).

Correspondingly, Powell (2012) has shown that military coup d'états become more likely with a rising level of mass threats.[18] Thus, if we do not consider the initial strength of the opposition, we risk confounding effects of political institutions with effects of opposition strength.

Furthermore, we risk inducing omitted variable bias. When considering the effects of regime type, for instance, discernible effects of institutions may in fact be attributable to other factors that affect the opposition's strength and the opposition's leverage against the regime, such as the level of manufacturing to GDP, the extent of urbanization or the level of mineral exports (Butcher and Svensson 2014; Dahl et al. 2014). Recognizing the potential effect of contextual variables is crucial if we are to establish any independent effects of political institutions. Accordingly, the fundamental question we have to ask is whether, once established, the institutional set-up of a regime has an *independent* effect on the probability of nonviolent campaign onsets.

[18] Mass threats is operationalized as the presence of assassinations, purging of governmental officials, guerilla activity, protests, riots, and strikes as coded by Banks (2001).

3.2 Party-based Co-optation and the Threat of Rebellion

In order to explain the dynamic between opposition strength, political party restrictions and nonviolent uprisings, I use the theory of Gandhi and Przeworski (2006) and Gandhi (2008) as a theoretical backdrop. Their theory is particularly relevant for my purposes, given their focus on popular uprisings. The crux of this argument is that dictators respond to an increased level of mass threats with political parties, and that these institutions in turn help dictators encourage cooperation and thwart rebellion. The assumption is that parties emerge as a best response when there is a strong and polarized opposition. Dictators will be reluctant to rely on political parties because they widen the number of people eligible for sharing the spoils of office. Thus, when faced with weak opposition the dictator may see no need for political parties: the dictator can just as easily distribute spoils without these institutions. In contrast, when the dictator is more dependent on cooperation and faced with stronger opposition, dictators frequently rely on political parties to facilitate co-optation (Gandhi and Przeworski 2006). Figure 3.1 provides an example of how this relationship can be modeled graphically.

The figure shows that both the likelihood of mass rebellion and the degree of institutionalization are affected by the initial strength of the opposition. It suggests that while opposition strength has a direct and positive influence on the likelihood of an uprising it also has countervailing negative influence through its effect on the level of institutionalization. Note that in this simplified version of Gandhi and Przeworski's (2006) model the need for cooperation is assumed to fall under the larger rubric of opposition strength: regimes that are less dependent on cooperation also tend to face weaker opposition.[19] Otherwise, opposition strength is affected by factors such as the degree to which the opposition is organized, the stability of the regime, the level of repression and the international environment (Gandhi 2008: 168). Crucially, Gandhi and Przeworski's (2006) model posits that greater reliance on legislative parties serve to neutralize the initial increase in risk associated with confronting a stronger opposition. Thus, there should be no empirically discernible effect of the number of political parties on the likelihood of nonviolent uprisings.

[19] This point is illustrated by again considering the example of rentier states. First, the population has only limited involvement in the production of wealth in these countries, which gives the opposition limited leverage vis-à-vis the regime. Second of all, these economies typically do not see the same level of organizational profusion as for instance countries that depend on manufacturing. The spread of organizations such as labor unions has been argued to be essential for mobilizing the opposition, but these are typically less frequent in rentier states (Butcher and Svensson 2014).

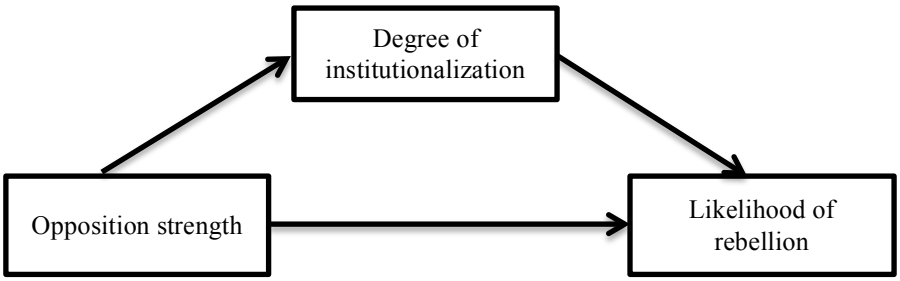


Figure 3.1 *Gandhi and Przeworski's (2006) model of political survival (a simplified version)*

The hypothesis that dictators respond with greater levels of institutionalization in the face of a powerful opposition is widely recognized in the literature on authoritarian survival (e.g. Boix and Svolik 2013; Bueno de Mesquita et al. 2003; Magaloni and Kricheli 2010; Smith 2005). Yet, as emphasized in Chapter 2, there are strong reasons not to expect a completely uniform effect of opposition strength on the level of party institutionalization. As Magaloni and Kricheli (2010: 127) note, the dictator will be reluctant to broaden institutional access to the regime when the elites' threats to the dictator's rule are credible. Because permitting opposition parties is associated with a higher risk of internal defections, dictators should be biased against reforms towards multiparty autocracy. In particular, if the threat of being replaced through a coup d'état has historically been larger than the relative threat of being deposed by a popular uprising, then this also suggests that single-party regimes will tend to 'underinstitutionalize' in the face of a stronger opposition. Hence, we should be able to trace empirically discernible effects of the level of institutionalization on the likelihood of nonviolent uprisings.

3.3 The Dictator's Dilemma: Reform and Succumb

This section presents the theoretical rationale for the main unconditional model, which identifies single-party regimes as particularly prone to nonviolent uprisings. I start with a simple model of popular uprisings, where I consider the role of three actors: the dictator, the ruling coalition and the opposition. The choice between violent and nonviolent tactics is discussed in the next section. The ruling coalition is composed of the dictator's allies and make up the internal clique of the regime. It may include party members, military officers, family members, and other groups that are directly affiliated with the regime. The opposition is thus those segments of the population that are not directly affiliated with the regime, and

include both segments that are ideologically proximate and ideologically distant to the regime.

Following Gandhi and Przeworski (2006) and Gandhi (2008), I assume that no-party regimes, single-party regimes and multiparty autocracies differ in terms of their ability to co-opt the opposition. Political parties are instrumental in mobilizing mass support. By including a larger group among the politically enfranchised, party regimes allow more people to have a say in politics, even if only in restricted forums (Gandhi 2008). Furthermore, by controlling the access to power, positions and rents through regime-sanctioned parties, party regimes also establish extensive patronage networks to attract those segments of the population that are ideologically close to the regime (Magaloni and Kricheli 2010; Svolik 2012). However, by legalizing opposition parties, multiparty autocracies also become more efficient at co-opting those segments of the population that are not ideologically proximate to the regime. Handing the opposition institutional autonomy serves to regularize political interaction and lowers uncertainty over whether the opposition will be included in future bargaining with the regime (Gandhi 2008). Crucially, it also gives the opposition a forum through which it can proclaim dissent and mobilize supporters without incurring the high costs that are associated with organizing a popular uprising, which often are met with harsh regime repression (Magaloni 2008).

Furthermore, I follow Gandhi and Przeworski (2006) and Gandhi (2008) in assuming that dictatorships ban parties when the opposition is weak and that they allow political parties when the opposition is moderately strong or strong. Both maintaining elite cohesion and mass cooperation becomes more difficult as the opposition grows stronger. As Magaloni and Krucheli (2010: 137) note, "[m]ilitary officers are more likely to defect the regime if they perceive the masses of being capable of orchestrating a revolution, even a peaceful one." Dictators therefore tend to rely on political parties under conditions that boost opposition strength, such as when a country is more dependent on domestic investments, less reliant on mineral exports and has historically had a lower level of repression (Gandhi and Przeworski 2006; Wright 2008). Yet, these countries remain authoritarian because the opposition is not sufficiently strong to consolidate or push for a full transition to democracy (Levitsky and Way 2002: 60-61). The main exception to this pattern are military regimes, which frequently place severe restrictions on political parties, but tend to emerge when dictators are confronted with strong and polarized opposition (Magaloni 2008; Svolik 2012).

Yet, I depart from Gandhi and Przeworski (2006) and Gandhi (2008) in one crucial respect: I do not expect dictators in single-party regimes to automatically respond with

legalizing opposition parties in the face of a stronger opposition. Allowing opposition parties carries the risk of provoking internal defections, as potential opponents within the regime are presented with a peaceful venue for challenging the dictator (Magaloni 2008). This heightens tensions between the dictator and his ruling coalition, and increases the likelihood that the dictator will be removed, either through a coup d'état or through an election. As such, dictators will have an incentive to delay the broadening of political participation. As Bratton and van de Walle (1997) have observed in sub-Saharan Africa, "[democratic] transitions in Africa seem to be occurring more commonly from below" because rulers "resist political openings for as long as possible and seek to manage the process of transition only after it has been forced on them". Similarly, Magaloni and Kricheli (2010: 127) have emphasized how "[the] dictator has an incentive to broaden his supporting coalition *only* when the opposition is powerful enough to threaten the stability of the regime".[20]

Table 3.1 *Opposition strength, institutions, and the likelihood of popular uprisings*

	Degree of Institutionalization		
	Parties banned	**Single party**	**Multiple parties**
Opposition strength	Low	Medium/High	High
Ability to co-opt opposition	Low	Medium	High
Likelihood of popular uprising	Low	High	Low

Table 3.1 summarizes these hypotheses. Unlike Gandhi and Przeworski (2006) and Gandhi (2008) I do not anticipate that dictators to automatically respond with a sufficient level of institutionalization to neutralize dissent, even if we assume that elections and the security apparatus enable the dictator to observe the relative need to institutionalize.[21] The reason is that the dictator has to weight the relative loss in terms of internal cohesion against the relative gains of co-opting the opposition. The implication is that single-party regimes will tend to 'underinstitutionalize' in the face of a strong opposition. Therefore, single-party regimes face a higher likelihood of popular uprisings because their institutions are not designed to co-opt the ideologically distant opposition. In contrast, multiparty autocracies feature institutions that allow them to counteract the threat from a stronger opposition.

Consider the case of Kenya's nonviolent campaign from January 1990 until December 1991. Before the uprisings, Kenyans had seen years of single-party rule under the leadership

[20] My emphasis.

[21] This latter assumption is also somewhat optimistic, as regime insiders may be liable to understate the strength of the opposition if they secretly prefer political change (e.g. Kuran 1989).

of the Kenyan African National Union (KANU). The country had been a de facto one party regime ever since the days of President Jomo Kenyatta (1963-1978), and became a de jure one in 1982, during the early years of President Daniel arap Moi's tenure (Nepstad 2011: 97-98). Following these restrictions, and later moves to expel party members that deviated from KANU positions in 1985, protests were soon starting to mobilize both in universities and churches. Nepstad (2011: 98) tells the story of how nonviolent action came to transpire in the lack of institutional channels to challenge the dictator:

> As Moi became increasingly dictatorial, those who sought change could not organize against him through institutional means since opposition parties were prohibited. Therefore, they had to find a free space where they could cultivate resistance and mobilize action. They found this space in universities and churches. (Nepstad 2011: 98)

After having encouraged the opposition to boycott the 1988 elections, church leaders asked Kenyans in 1990 to emulate East Germany's recent nonviolent revolution (Nepstad 2011: 101). Following nearly two years of protests, and significant pressure from both domestic and international sources, Moi announced in December 1991 to legalize opposition parties and hold multiparty elections in 1992, which spurred the end of the uprisings (Nepstad 2011: 103-104).

Subsequently, the opposition began to organize through institutional channels and formed the Forum for the Restoration of Democracy (FORD). Yet, internal divisions soon emerged based on ethnic tensions and class divisions, and FORD was consequently split into three parties. Despite winning popular majorities in the 1992 and 1997 elections, the opposition parties were unable to oust Moi from office. Each party remained determined to win the elections on their own accord (Nepstad 2011: 104-106). Thus, in spite of systematic government repression and harassment of the opposition, the opposition did not mobilize any new uprisings. Consequently, Moi was able to rule uninterrupted until he retired in 2002 (Schedler 2002b: 106-108). When the opposition was finally able to unite and prevent Moi's appointed successor, Uhru Kenyatta, from taking office, this was done through popular elections rather than non-routine direct action.

An equal pattern was found in Bratton and van de Walle's (1997) study of political protests over regime change in sub-Saharan Africa between 1985 and 1994.[22] Their study revealed that such protests were most frequent in single-party regimes. As they argue, "because in practice single-party monopolies blocked or restricted the expression of popular preferences, citizens had little choice but to experiment with informal, even extralegal, modes of participation outside of official party or legislative channels" (Bratton and van de Walle 1997: 144-145). Similarly, several years of civilian protests preceded the transition from a single-party to a multiparty autocracy in Taiwan (Solinger 2001: 33-34). As Schock (2005: 119) observed in the case of The Tianmen Square Democracy Movement in China and the nonviolent uprisings against the Burmese military in the late 1980s, "nonviolent action may be the prevailing means by which an oppositional civil society asserts itself and then defends itself from state repression, thus promoting democratization".

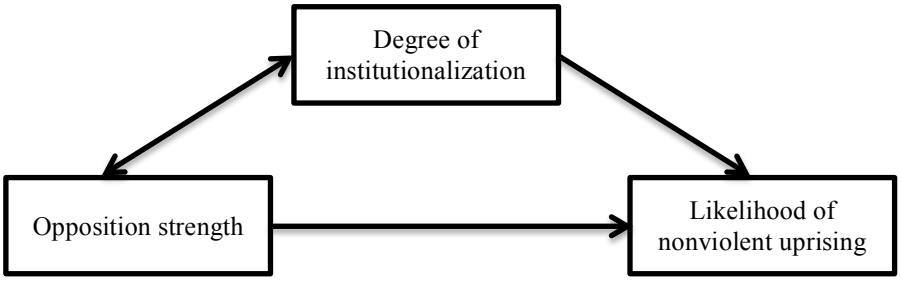


Figure 3.2 *A model of the relationship between opposition strength, institutions and the likelihood of nonviolent campaign onsets*

Furthermore, we also have to consider the possibility that institutions affect the relative strength of the opposition, and not just vice versa. Figure 3.2 displays this relationship graphically. For instance, in their study of protests in sub-Saharan Africa, Bratton and van de Walle (1997) found that party regimes encouraged much higher levels of pro-regime participation. Elections were a regular feature in most party regimes, even if most elections were primarily symbolic and served to endorse the ruling party's candidates. Similarly, several leaders also promoted carefully orchestrated mass demonstrations as means of

[22] Bratton and van de Walle (1997: 128-129) define protests as "the frequency of mass actions directed at political goals. Qualifiying for inclusion are street demonstrations, boycotts, strikes, and riots. A mass action has a political purpose when protesters make explicit demands for changes in political rights or rulership. Political protests are thereby distinguished from mass actions drivene exculsively by economic grievances". Arguably this definition is quite close to what I term governmental nonviolent campaigns, the main difference being that these protests are not necessarily as sustained and organized as campaigns, and that the latter excludes riots.

allowing citizens to let off steam. Yet, in the absence of political competition, increased political participation eventually caused frustration and encouraged citizens to pursue nonviolent action. As the authors argue, "desiring greater political competition, former voters and nonvoters therefore sought alternative channels of participation outside of elections. One such manifestation was protest action" (Bratton and van de Walle 1997: 142). Elections and public gatherings are noted as regular features of single-party regimes also outside Africa (Magaloni and Kricheli 2010: 129). Importantly, this argument carries the same implications as in Table 3.1. Unless the regime possesses the institutional means to channel a stronger opposition, then dictatorships should see a higher likelihood of a popular uprising with increased participation.

Hypothesis 1 *Single-party regimes run a higher risk of nonviolent campaign onset than both multiparty autocracies and no-party regimes.*

Finally, Gandhi and Przeworski's (2006) model also suggests that the likelihood of an uprising should be no higher in multiparty autocracies than no-party regimes. The idea is that multiparty autocracies are able to respond with appropriate levels of institutionalization to neutralize the threat of a much stronger opposition. Thus, although dictatorships that ban parties face considerably weaker opposition movements than multiparty autocracies, no-party regimes have fewer means than multiparty autocracies to prevent protests from escalating into potentially regime destabilizing activities. This assumption appears plausible.

On the one hand, permitting opposition parties gives a larger share of the opposition incentives to make their demands within dictatorial institutions rather than on the street. Participating in protests could lead dictators to revoke domestic groups' privileged status and access to decision-making. As Lust-Okar (2005) has shown in the cases of Egypt, Jordan, and Morocco, opposition parties refrained from mobilizing the masses in times of economic crises, even though they possessed the means to do so. According to the author this was precisely out of fear that these groups would lose their status with the elites.

Conversely, if party elites possess a significant political infrastructure for mobilizing dissent, expelling political parties should increase these groups' incentives to rebel. This was evident in both the Philippines and Nepal, where opposition parties prevailed as underground organizations despite being banned under the constitution, and were instrumental in organizing the nonviolent campaigns that took place in 1986 and 1990 (Nepstad 2011: 110-118; Schock 2005: 69-79; Teorell 2010: 111). This suggests that allowing multiple parties

could be important in sidelining reformist elites that otherwise would be inclined to mobilize an uprising (Nepstad 2011: 110-118; Schock 2005: 69-79, 121-125).

Finally, we also have to consider that most transitions from no-party regimes have been to multiparty autocracies rather than single-party regimes (Magaloni and Kricheli 2010). In the cases of Morocco and Jordan, for instance, moderate opposition groups were allowed formal access to the political system precisely as a means to split the opposition in periods of economic distress (Lust-Okar 2005). In both of these cases the transitions to multiparty autocracy came as direct transitions from no-party rule. Thus, although it appears likely that regimes respond with greater levels of institutionalization to neutralize popular dissent, it seems less likely that this route will involve transitions to single-party rule.[23]

Hypothesis 2 *Multiparty autocracies run no higher risk of nonviolent campaign onset than no-party regimes.*

3.3.1 Precluding Nonviolent Mobilization

A possible objection to the hypotheses above relates to Hanne Fjelde's (2010) discovery that single-party regimes experience a lower likelihood of armed conflict than multiparty autocracies. Given that I expect the reverse pattern for nonviolent campaigns, my hypotheses could potentially be at odds with her findings. To explain why we should expect single-party autocracies to be more exposed to *nonviolent* movements in particular - as opposed to violent methods of direct action – I therefore believe we should disaggregate our conception of the opposition. In particular, I suggest that we have to consider the roles of co-optation and repression in lieu of variations in actor constellations within the opposition. Once we take into account these variations it becomes more evident that multiparty autocracies should at least temporarily exacerbate collective action problems among groups that typically rely on nonviolent direct action.

Actor Constellations, Co-optation and Nonviolent Uprisings

Most studies of authoritarian survival and democratic transitions treat the opposition as a unitary actor (e.g. Fjelde 2010; Gandhi 2008). However, to grasp the dynamics of party co-optation and nonviolent mobilization, I believe it is necessary to relax this assumption.

[23] Data from Wahman et al. (2013), for instance, suggest that there were 26 transitions from no-party regimes to single-party regimes, and 68 transitions from no-party regimes to multiparty autocracies in the period 1972-2006.

Different elements of the opposition may pursue very different and conflicting goals, may utilize completely different tactics, and may never come to terms with each other. For instance, according to Celestino and Gleditsch (2013: 390), "predominantly violent and nonviolent tactics tend to be used by different groups and often entail very different recruitment bases and opportunities for growth". Similarly, Dahl et al. (2014: 5) observe that a group's initial choice of nonviolent or violent tactics tend to be "remarkably stable", and argue that group motives play a key role in a group's initial choice of tactic, whether violent or nonviolent. Crucially, differentiating opposition groups is important here because the effectiveness of co-optation strategies may differ depending on the goals of the dissidents.

First, as Gandhi and Lust-Okar (2009) note, compared to incumbent candidates, opposition candidates in multiparty autocracies primarily attract urban, middle- and upper-class voters. Typically, these voters tend to be highly ideological, and strongly disagree with current policies (Gandhi and Lust-Okar 2009; Greene 2007). Importantly, nonviolent campaigns tend to recruit from the same group of citizens. According to Celestino and Gleditsch (2013) note, "Nonviolent campaigns are often urban phenomena, which recruit disproportionally from students, individuals of the middle classes, or included social segments". Similarly, Welzel and Inglehart (2008) emphasize how political activism and democratic movements tend to concentrate in urban areas and rise in tandem with economic development.

By contrast, incumbent candidates in single-party and dominant-party regimes are more likely to receive electoral support in rural areas than in urban areas (Gandhi and Lust-Okar 2009). Especially in single-party regime we see that regime parties tend to maintain extensive patronage networks that reward loyal citizens and withdraw rents from those who defect (Magaloni and Kricheli 2010; Smith 2005). As Magaloni and Kricheli (2010: 128) contend, these "punishment regimes" are "particularly effective at trapping poor voters into supporting the dictatorship, because their livelihood depends on state transfers". In comparison, "[citizens] with alternative sources of income can better afford to make 'ideological investments' in democratization and oppose the regime." These considerations become especially important given the fact that civil wars tend to be fought in the periphery, and that rebels are disproportionally recruited from the bottom income quartiles (Buhaug, Cederman, and Rød 2008; Collier and Hoeffler 2004). Thus, although single-party regimes may be more effective in co-opting groups that potentially could resort to violent tactics, they should still be less effective than multiparty autocracies in appeasing those opposition groups that typically resort to nonviolent tactics.

Furthermore, we also have to consider who are targeted for political inclusion in multiparty autocracies, as compared to single-party regimes. Apart from the political inclusion of opposition elites, multiparty autocracies also tend to include a broader diversity of labor interests in political bargaining with the regime (Kim and Gandhi 2010). Both are groups that have been noted as important in the organization and success of several nonviolent campaigns (Butcher and Svensson 2014; Nepstad 2011; Schock 2005). Furthermore, there are several examples of political parties that have emerged from broad-based social movements, such as the Justice and Development Party (PJD) in Morocco, the Muslim Brotherhood in Egypt, and the Islamic Action Front (IAF) in Jordan (Gandhi and Lust-Okar 2009). Conversely, radical segments of the opposition are typically not targeted for co-optation. Radical Islamist parties, for example, have for extended periods been legally proscribed in Egypt, Tunisia, and Algeria (Sivan 2000: 77). Crucially, as the cases of Egypt and Algeria illustrate, some of these movements have resorted to violence rather than nonviolence in the face of exclusion (Diamond 2010).

Finally, other groups could conceivably have no interest in participating through nominally democratic institutions. Groups pursuing the radical transformation of society as whole – such as the transition to Communism or the imposition of Sharia law – may find it impossible to do so by any other means than armed struggle. For instance, the armed wing of the Communist party of the Philippines (CPP), the New People's Army (NPA), pursued an explicit and agreed-upon strategy of armed insurrection to gain power. This contrasted with the reformist and progressive elements of the opposition, which mainly used nonviolent methods of resistance (Schock 2005: 67-69). While the People's Power Movement soon dismantled after the ousting of President Marcos,[24] the NPA continued fighting the new government and remain active today (Themnér and Wallensteen 2014).

Actor Constellations, Repression and Nonviolent Uprisings

To reiterate, multiparty autocracies are distinct from democracies. Although formal democratic institutions are perceived to be the principal means of obtaining and exercising political authority, multiparty autocracies "violate those rules so often and to such an extent […] that the regime fails to meet conventional minimum standards for democracy" (Levitsky and Way 2002: 53). Manipulation is a defining feature of multiparty autocracies. Unlike

[24] A Second People's Power Movement was first mobilized 14 years later to oust President Joseph Estrada, after allegations of massive fraud (Törnquist, Tharakan and Quimpo 2009: 200-202).

elections in democracies, elections in these regimes are subject to extensive authoritarian control.

Yet, manipulation in multiparty autocracies usually tends to take more subtle forms than the kind of repression witnessed in closed dictatorships (Schedler 2009: 385). For instance, dictatorships with lower levels of party institutionalization generally tend to have lower restrictions on civil liberties (Frantz and Kendall-Taylor 2014; Gandhi 2008). By contrast, because multiparty autocracies usually face restrictions on the extrajudicial means available to eliminate subversive elements, they have tended to employ alternative tactics to manipulate the opposition (Levitsky and Way 2002). These tactics include formal measures that give the incumbent an unjust advantage, such as the radical rules of proportional representation in Nicaragua, or the majoritarian electoral rules in post-revolutionary Mexico and Zimbabwe under Mugabe (Schedler 2002a: 45, 2002b: 107). Crucially, one tendency is for multiparty autocracies to exploit ethnic and religious cleavages that previously had not been politicized (Snyder 2000). In Kenya, for instance, government-financed death squads were actively involved in fueling 'tribal conflicts' prior to the elections in 1992, something president Moi used as proof of his prophecy that multipartyism would lead to 'tribal conflict' (Brown 2001; Schedler 2002b: 108). Another prominent tactic is the harassment of opposition leaders and other members of civil society, such as the systematic violence against opposition candidates, civil society organizations and independent media outlets in Zimbabwe during the election in 2000 (Diamond 2002: 32; Schedler 2002a: 44). While some of these strategies lead to increased violence, they frequently also generate divided opposition movements (Lust-Okar 2005; Magaloni 2010).

In general, nonviolent action against a government requires great numbers and hinges on the expectation that others will participate (Chenoweth and Stephan 2011). Thus, if a significant share of the opposition regard conventional channels as an attractive alternative to nonviolent action, or simply are unable to coordinate, major nonviolent campaigns should be less likely to mobilize. By contrast, most armed conflicts in multiparty autocracies tend to be of low intensity, involve small groups and are fought over territorial disputes. It is often less costly to target these groups than the liberal opposition, which typically have greater leverage towards the regime because they control key sources of economic activity (Celestino and Gleditsch 2013). Coupled with the higher likelihood of coup d'états, it is therefore hardly surprising that multiparty autocracies experience a higher likelihood of armed conflicts than dictatorships with single parties. Yet, as I have shown, this does not imply that multiparty

autocracies should be more prone to nonviolent conflict in general. However, what I have not considered so far is how regime duration factors into this calculation.

3.4 When Does the Opposition Coalesce?

Should we expect the effects of restrictions on political parties to be constant over time, or is it conceivable that the effects of these restrictions are conditioned on the proximity to the last regime change? This section presents the theoretical rationale for the main conditional model, which stipulates that the impact of political party restrictions on nonviolent conflict is conditioned on the duration of a regime. Manipulation may to some extent help authoritarian leaders remain in power and forestall democratization, as the Kenyan case illustrates. Yet - just as other forms of repression - manipulation also carries a risk of backfiring. The final suggestion I make here is that manipulation can only go so far before it backfires and serves to unite the opposition, a process that is commonly referred to by civil resistance scholars as "political jiu-jitsu".[25] This typically happens when electoral manipulation and oppression of the opposition becomes so blatant that opposition parties deem it impossible to achieve regime change without nonviolent action. In the literature these events are commonly referred to as 'electoral revolutions' (Bunce and Wolchik 2006; Nepstad 2011: 11-12; Schedler 2009: 388). These are events where activists nonviolently organize "to transform rigged electoral rituals into fair elections, thereby facilitating a transition from an illiberal to a more liberal government" (Bunce and Wolchik 2006: 6). Nonviolent action may be mobilized both before the balloting and to contest the final results. Examples include the nonviolent campaigns in Mexico, The Philippines, Indonesia and Peru, as well as the 'color revolutions' in Serbia, Georgia, Ukraine and Kyrgyzstan (Bunce and Wolchik 2006: 5-6; Nepstad 2011: 13).

Following Schedler (2002b), I suggest that the dynamics of electoral revolutions can be modeled as "nested games", where "the game in the principal arena is nested inside a bigger game where the rules of the game themselves are variable" (Tsebelis 1990: 8). At the same time as the incumbents and the opposition compete in the electoral arena, they battle over the basic rules that shape the electoral arena. The opposition accept the rules of the electoral game as a temporary compromise, "a provisional truce contingent on current correlations of

[25] According to Nepstad (2011: 15) political jiu-jitsu "denotes the paradoxical consequences that occur when regimes use coercive measures against an unarmed population. Brutal sanctions expose the regime's viciousness, causing sympathy for the nonviolent resisters to increase and support for the ruler to decrease—both domestically and internationally.

force and open to revision in the uncertain future (2002b: 109). Their acceptance of semi-authoritarian elections is conditioned on their ability to win elections. Thus, even if the odds are deliberately stacked against the opposition, semi-authoritarian elections may involve sufficient uncertainty for the opposition to invest in them.

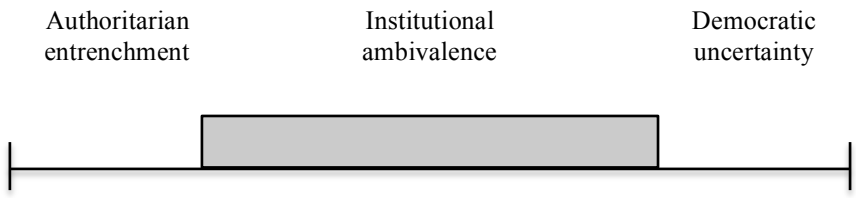


Figure 3.3 *The Ambivalence of Multiparty Autocracy.*[26]

Yet, as Schedler (2002b: 114) notes, "opposition parties face the dilemma that democratizing the pre-democratic electoral game often goes hand in hand with legitimizing it". Consequently, although elections may potentially be meaningful, participating in them also involves legitimizing authoritarian manipulation. While elections give opposition parties the opportunity to mobilize strength, they also confer the dictator with democratic legitimacy. In the absence of meaningful institutional change or electoral gains this ambivalence is amplified. In the presence of overt manipulation, elections are in practice reduced to authoritarian façades (see Figure 2). It is under these circumstances the opposition is likely to unite in seeking alternative measures, either to directly overturn the dictator or to manage the ambivalence of elections (Schedler 2002b: 117). As Nicolas van de Walle (2006: 78) suggested when commenting on civilian protests in sub-Saharan Africa in the 1990s, "[in] many cases, it was precisely the fact the elections were unwinnable that led the opposition to favor extralegal means to gain power."

Based on this logic, I expect the effect of permitting multiple parties to be conditioned on the time since the last regime change, and multiparty autocracies to become more prone to nonviolent uprisings over time. Importantly, this expectation contrasts with the common belief that established dictatorships are more capable of handling contention (e.g. Bueno de Mesquita et al. 2003; Svolik 2009, 2012). According to this conception instability is what drives contention, and dictators become less exposed to contentious actions as institutions become consolidated. Chenoweth and Ulfelder (2015: 12), for instance, suggest that "the longer a regime remains in power, the less vulnerable the regime is to domestic challenges.

[26] Note: This figure is partly based on a model by Schedler (2002b: 109).

This is because regimes that endure over generations may have a depressing effect on peoples' expectations about the utility of nonviolent action in challenging the regime's grip on power". What I posit here is in many ways the exact opposite; it is not instability *per se* that drives nonviolent uprisings, but rather the fear that authoritarian rule will become consolidated and stable. Crucially, in the case of multiparty autocracies I expect this phase to be in the later stages of a regime's tenure rather that the initial phase following regime change.

After the transition to multiparty elections it may be hard to determine whether election failure is manufactured or due to the incompetence of the opposition, and the opposition will have few incentives to mobilize outside conventional channels (Schedler 2002a: 42). The opposition is likely to have limited experience with party politics, and may use some time to build sufficient infrastructure to challenge the dictator in the electoral arena (Schedler 2002b: 106). As illustrated by the initial years of multiparty competition in the post-communist countries of Central and Eastern Europe, a myriad of different parties will contend for office and voters are likely to be highly uncoordinated (Enyedi and Bértoa 2011). In many cases, restricted access to media and public funds also make electoral mobilization difficult, as opposition parties are dependent on mobilizing sufficient support also outside the capital (Levitsky and Way 2002: 57-58; Schedler 2002a: 43-44).

Furthermore, protesting results requires substantial resources and mobilization strength, and the opposition parties will have to supply credible information about the actual results to convince voters of fraud (Magaloni 2010). This message is unlikely to stir any major response if opposition parties are fragmented and the electoral margin is large. Even if the opposition is somehow able to monitor the results, their reports are unlikely to be broadcasted. Thus, as Magaloni (2010: 761) observes, "partially rigged elections for which there is no clear public signal about fraud make it hard for society to coordinate against potential dictatorial abuses". Fearon (2011: 1676) reaches a similar conclusion, arguing "it is the commonly understood convention of holding elections at particular times according to known rules, not the electoral outcome itself, that provides a public signal for coordinating rebellion in the event that elections are suspended or blatantly rigged". Hence, in the early years following a regime transition incumbents are not dependent on using significant coercion to stay in power; selective manipulation may be sufficient to prolong their stay (Levitsky and Way 2002).

Eventually, however, voters become more coordinated, and some opposition parties attain a significant enough electoral presence to constitute genuine challengers for office. As

opposition parties gain experience and electoral presence, they are also more likely to be able to monitor results and broadcast them. If elections are then repeatedly revealed as shams and the opposition experiences prolonged periods without any meaningful moves towards democracy, then the opposition should be inclined to seek alternative measures to increase competition. Furthermore, as Levitsky and Way (2002: 59) contends, it is under such periods of serious democratic contestation that dictators are most likely to rely on overt manipulation; otherwise they risk electoral defeat. Yet, as noted previously, repression carries a high risk of backfiring, especially now that the opposition can utilize its increased electoral presence to mobilize greater numbers and exercise greater leverage. Under these conditions, combining electoral participation with nonviolent action may be the only means of achieving a full transition to democracy. One notable example is the wave of protests in the early 1980s and the mid-1990s against the dominance of PRI in Mexico, where the opposition actively combined electoral participation with post-electoral protests (Schedler 2002b: 117; Solinger 2001). Similarly, the democracy movement in South Korea actively combined strategies of nonviolent action with electoral participation in the years preceding the transition to democracy in 1987 (Shorrock 1986; Solinger 2001).

A prevalent alternative to the scenario outlined above is that opposition parties remain small and fragmented, and regime parties become able to cement their hold. Under such circumstances, voters typically become increasingly disillusioned by electoral politics, coming to view opposition parties as politically toothless or mere extensions of the authoritarian regime. Importantly, as the nonviolent uprisings in Tunisia and Egypt in 2011 illustrate, such a development will often also generate a civilian response. For years the regime parties in Tunisia and Egypt had won a clear majority of parliamentary seats: repeatedly above 80 percent in Tunisia, and consistently above 70 percent in Egypt (Diamond 2010).[27] Under such circumstances multiparty autocracies come to resemble one-party regimes, which – as the theoretical framework stipulates – should be associated with a higher risk of nonviolent uprisings. Although these cases are not 'electoral revolutions', they nevertheless produce the same general expectation, namely that the probability of a nonviolent uprising in multiparty autocracies should increase over time.

Conversely, in the case of non-competitive regimes, we should expect the fear of authoritarian entrenchment to generate more contention immediately following a regime change. After transitions to no-party regimes, dictatorship is quickly apparent, and the fear of

[27] Figures also drawn from Wahman et al. (2013).

authoritarian consolidation is present from day one. Granted that the opposition is strong enough, it has incentives to mobilize immediately. This is more in line with the popular notion of how regime duration affects contentious politics (e.g. Chenoweth and Ulfelder 2015). Military regimes, for instance, often face significant pressures to democratize, and have almost ceased to exist as anything more than a transitional type of rule (Diamond 2002: 27). In no-party regimes longer duration is therefore often a source of stability for the incumbent rather than instability; it is a sign that the regime has successfully implemented authoritarian control. Monarchies that have established dynastic succession, for instance, experience remarkably long tenures and relatively few threats to their rule (Hadenius and Teorell 2007; Herb 1999).

Finally, in single-party autocracies there are competing tendencies. On the one hand these regimes are unmistakably authoritarian. Following a transition the opposition may therefore have incentives to mobilize supporters before the regime consolidates authoritarian control. Yet single-party regimes are more inclusive than no-party regimes, and may constitute an improvement to previous practices (Gandhi and Przeworski 2006). Moreover, because the electoral practices in many one-party regimes also tend to boost opposition strength, the risk of popular mobilization may increase during their lifespan (Bratton and van de Walle 1997). It is therefore unclear when the opposition is most likely to mobilize in one-party regimes. Nonetheless, the general expectation is the same: with longer regime duration the risk of a nonviolent campaign onset in a multiparty autocracy should approach the risk of an onset in other autocracies.

Hypothesis 3: *The impact of regime duration on nonviolent campaign onsets is higher in multiparty autocracies than in no-party and single-party regimes.*

3.5 Summary

In this chapter I have presented a theoretical framework for understanding the relationship between political institutions and nonviolent campaigns. Drawing on theories on authoritarian survival and civil resistance, I suggest that the interaction between opposition strength and political institutions play a key role in determining the risk of a nonviolent uprising. In particular, I make two novel propositions: (1) single-party regimes will tend to 'underinstitutionalize' in the face of a stronger opposition, and (2) multiparty legislatures and elections are superior in coopting moderate segments of the opposition and are therefore

temporarily able to neutralize a greater threat of nonviolent revolution. The main implications are that (1) single-party regimes face a comparatively high risk of nonviolent campaign onset, (2) no-party regimes face a comparatively low and decreasing risk of onset, and (3) multiparty autocracies face a comparatively low but increasing risk of onset. Overall, the current framework suggests that institutions do matter with regards to nonviolent conflict because they provide different opportunities for channeling contention and precluding popular mobilization. In the next chapter I therefore proceed with presenting the research design, where I specify how these assumptions can be empirically tested. Table 3.2 provides a summary of the main hypotheses.

Table 3.2 *Summary of Hypotheses*

	Hypothesis
Main effects	**H1:** Single-party regimes run a higher risk of nonviolent campaign onsets than both multiparty autocracies and no-party regimes. **H2:** There is no significant difference in the likelihood of nonviolent campaign onsets between multiparty autocracies and no-party regimes.
Conditional effect	**H3:** The impact of regime duration on nonviolent campaign onsets is higher in multiparty autocracies than in no-party and single-party regimes.

4 Research design

In this chapter I develop a quantitative research design for testing the main expectations from Chapter 3, which suggest that the level of restrictions on political parties affect the mobilization of nonviolent uprisings. The purpose of the chapter is thus to bridge the gap between the theoretical framework and the empirical analysis. The chapter is organized as follows. First, I describe the main properties of the dataset and the operationalization of the dependent variable. Second, I present the logistic regression model, which is the statistical model employed in this study. Third, I discuss the operationalizations of the key independent variables and the specifications of the main regression models. Fourth, I discuss two methodological challenges that represent important threats to the validity of my results, namely endogeneity and missing data.

4.1 Dataset and Dependent variable

In order to test the hypotheses from Chapter 3 I rely on time-series cross sectional data with country-years as the unit of analysis. Country inclusion rests on membership in Gleditsch and Ward's (1999) list of independent nation states.

The dependent variable is a binary indicator denoting whether a nonviolent campaign is ongoing in a given year (1) or not (0). Data on nonviolent campaigns are drawn from the Nonviolent and Violent Campaigns and Outcomes (NAVCO) data, version 2.0 (Chenoweth and Lewis 2013b), which records the annual incidence of major nonviolent and violent campaigns in the period between 1945 and 2006. NAVCO 2.0 is an updated version of the original dataset used in Chenoweth and Stephan's study (2011), which was the first comprehensive cross-national dataset on all nonviolent campaigns. There exist no other relevant dataset with comparable coverage or reliability. The Domestic Conflict Event Data from Banks (2011) for instance, relies only on data from a single source – The New York Times - to code protest events. Moreover, protests are not necessarily sustained or intended to bring about regime change, which also rules out alternative sources such as the Social Conflict Analysis Database (SCAD).[28] What I am interested in here is strategic nonviolence,

[28] Although SCAD does specify categories for protests over elections, human rights and democracy, it focuses on protests which are not substantially in focus here. Moreover, the database also has limited coverage, only covering Africa and Latin America in the period 1990 to 2010.

which is distinct from singular events such as protests. The NAVCO data are unique in this respect, and also has the advantage that they are based on a specific sampling strategy and rigorous inclusion criteria.

In NAVCO, nonviolent campaigns have been identified from a broad range of sources, and then verified through multiple expert surveys. The coding rules are also well specified and transparent. To be classified as nonviolent, "the campaign must have been prosecuted by unarmed civilians who did not directly threaten or harm the physical well-being of their opponent" (Chenoweth and Lewis 2013b: 418). The campaign must rely almost uniformly on nonviolent methods, such as protests, strikes, boycotts or mass noncooperation. Furthermore, a campaign has to meet two additional criteria to be included in the dataset: goals and participation. First, there has to be proof of "a series of coordinated, contentious collective actions with at least 1,000 observed participants" (Chenoweth and Lewis 2013b: 417).[29] Second, only campaigns that at one point or another held 'maximalist' goals of regime change, secession, or the removal of a foreign occupier qualify for inclusion. For simplicity, I here refer to campaigns over regime change as governmental campaigns, and campaigns over secession or the removal of a foreign occupier as territorial campaigns.[30]

There are some potential issues concerning the coding of nonviolent campaigns, however. One issue is that the sample is biased toward successful campaigns. Campaigns that were defeated early on are underreported in the data. For instance, failed nonviolent campaigns may not be captured by the dataset due to extreme repression or limited news coverage, which would bias findings towards closed and repressive regimes. However, a similar underreporting bias exist also in the case of other contentious phenomena, such as armed conflicts (Chenoweth and Lewis 2013b: 420). This bias is to some extent ameliorated by applying a high participation threshold. The general implication is that any findings will only be applicable to 'major' campaigns that have a high level of sustained participation over time. Second, the inclusion criteria may be overly conservative: human rights movements for instance are excluded from the dataset. However, given that the substantive focus here is on regime change in a more narrow sense, I believe this more conservative approach is justified in my case. Third, coding campaign goals relies more on subjective evaluations than most of

[29] To be specific, a contentious event with 1,000 participants only qualifies as a campaign if it is "followed within a year by another contentious event with 1,000 observed participants claiming the same goals *and* there must be evidence of coordination across those events" (Chenoweth and Lewis 2013: 417).

[30] This is basically equivalent to the approach used in UCDP/PRIO's coding of armed conflicts, where a contested incompatibility may concern government and/or territory (Gleditsch, Wallensteen, Eriksson, Sollenberg, et al. 2002).

the other coding aspects in NAVCO, which can compromise reliability. Yet, this is a common concern in most quantitative assessments of political phenomena. Fourth, the coding of regime change may be biased towards more democratic regimes. For instance, this would be the case if nonviolent campaigns are systematically excluded from the data precisely because they take place in regimes that allow multiparty competition. This could critically compromise the validity of any findings regarding the association between political institutions and nonviolent campaigns. Nevertheless, I do believe that Chenoweth and Stephan's (2011) definition of regime change is sufficiently broad to ease any doubts. According to the authors, regime change "indicates a goal of overthrowing the state or substantially altering state institutions to the point that it would cause a de facto shift in the regime's hold on power" (Chenoweth and Lewis 2013a) They thus focus on cases where a movement aims to overthrow the existing regime, which is consistent with the theoretical focus here.

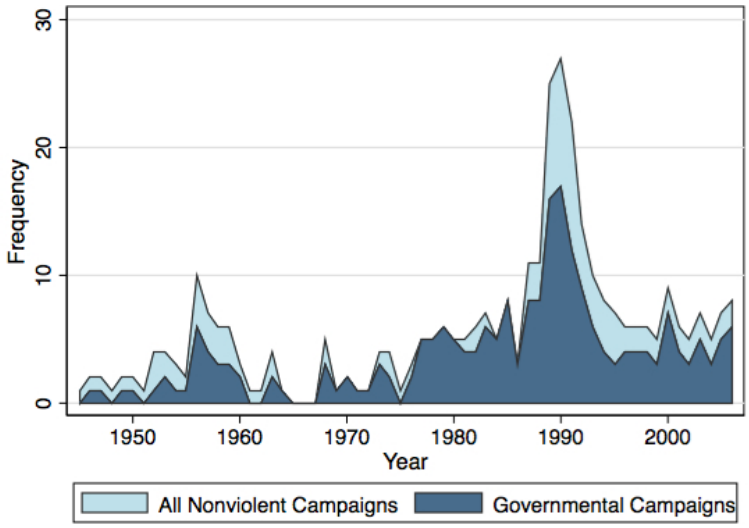
Overall, the operationalization of nonviolent campaigns thus corresponds closely with the theoretical conceptualization presented in the previous chapters, which is important with respect to content validity. The main concern in this respect is that the theoretical stipulations presented in Chapter 4 are primarily related to governmental campaigns, and not territorial campaigns. Yet, it has been suggested that similar mechanisms as those specified for governmental campaigns may also be applicable to territorial campaigns. Cunningham (2013), for instance, mentions how the lack of institutional political channels could provoke nonviolent campaigns in self-determination disputes. Running the analyses both with and without territorial campaigns is therefore important to determine whether the theoretical stipulations have any broader relevance outside governmental campaigns. Hence, I will run regressions using both the full sample, and a sample that excludes territorial campaigns.

In the present analysis I will compare nonviolent campaigns against the baseline "no event", instead of comparing nonviolent and violent campaigns. The rationale for doing so is mainly theoretical, as I am primarily concerned with the initiation of nonviolent campaigns. However, there is also the methodological concern that nonviolent campaigns and violent campaigns – as operationalized by Chenoweth and Stephan (2011) – may not be directly comparable. In contrast to nonviolent campaigns, violent campaigns are not included on the basis of the number of participants involved, but rather the number of battlefield-related deaths. Thus, it is questionable whether a threshold of 1,000 participants is analogous to a threshold off 1,000 battle deaths. I therefore believe a binary dependent variable is the most appropriate operationalization of the dependent variable, rather than a categorical variable

that includes both "non-events" and violent campaigns.[31] Nevertheless, I also run robustness checks with multinomial logit models (see Appendix D).

In order to collect country-years with no major nonviolent campaigns, I record all instances with available regime data but no nonviolent campaigns as "non-events". For this purpose I use the Authoritarian Regimes Data Set, version 5.0 (Wahman et al. 2013), as a baseline dataset. This requires transforming the original observations from campaign-year observations to country-year observations, similar to Butcher and Svensson (2014) and Dahl et al. (2014). The alternative is to identify groups that have been involved in conventional politics, but unfortunately there exists no such data that cover governmental disputes (Dahl et al. 2014: 32).[32]

Figure 4.1 *Frequency of nonviolent campaigns by year, 1945-2006*



Note: The figure shows the number of campaigns, not country-years. The category "All nonviolent campaigns" includes both governmental and territorial campaigns.

The resulting dataset covers a total of 193 countries and 5,943 country-year observations in the period between 1972 and 2006. The dataset is restricted to this period mainly because data on regime characteristics – including the main independent variables – starts in 1972. However, as figure 4.1 shows, relatively few campaigns (19) would be added by extending the study to 1960, which is the first year that I have data on all the control variables *and*

[31] Butcher and Svensson (2014), for instance, use a dependent variable with three outcomes: (1) no major challenge to the regime, (2) a violent resistance campaign, or (3) a nonviolent resistance campaign. Cunningham's (2014) operationalization of the dependent variable includes four outcomes: (1) largely conventional politics, (2) nonviolent campaign, (3) large-scale civil war, and (4) both nonviolence and civil war.

[32] Cunningham (2013), for instance, have used such an approach for her study of self-determination disputes, drawing data from the Center for International Development and Conflict Management (CIDCM) Peace and Conflict Report (Marshall and Gurr 2003). However, there exists no comparable data for governmental disputes.

alternative regime measures. This leaves 244 country-years with ongoing nonviolent campaigns, of which 171 are country-years with governmental campaigns, 67 are country-years with territorial campaigns, and 6 are country-years with both. [33]

4.2 Statistical Model

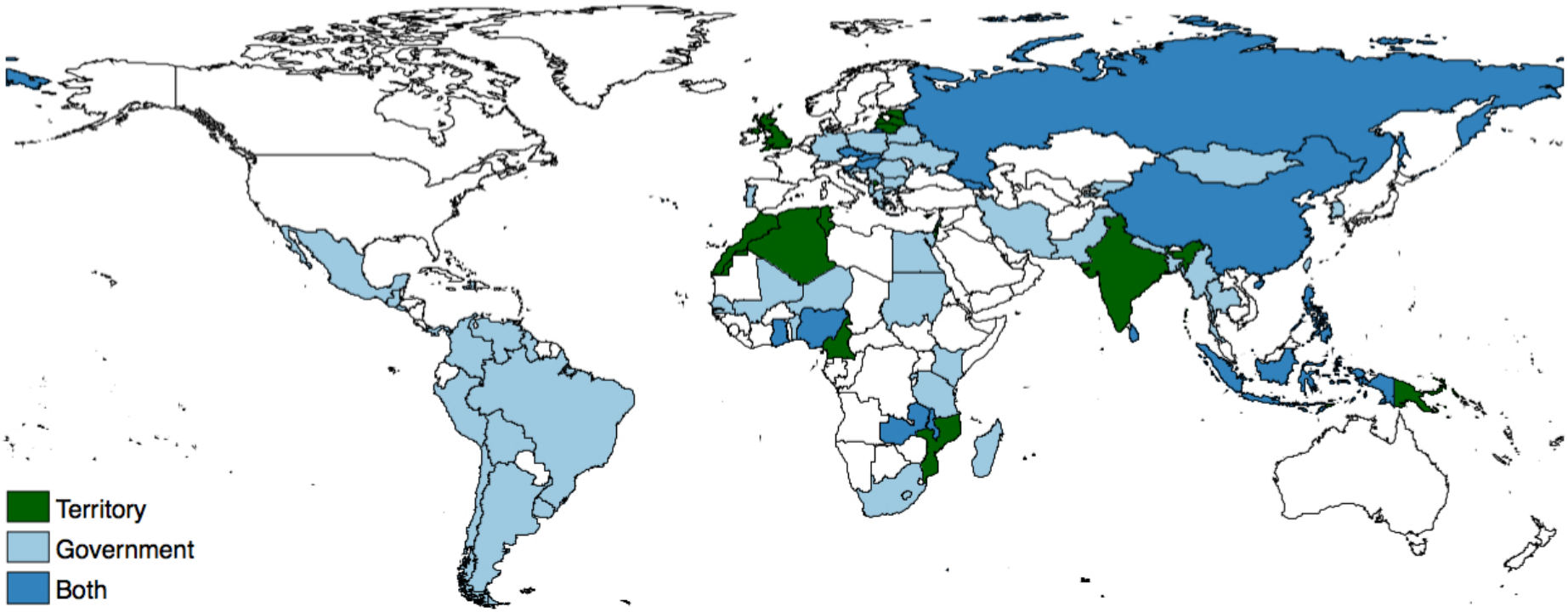
The main focus of this thesis is the onset of nonviolent campaigns. As the dependent variable is dichotomous, I have chosen to rely on a logistic regression to perform the empirical analyses. In terms of estimating binary outcome variables, the logistic regression model has some notable advantages over the standard Ordinary Least Squares (OLS) model. First, it assumes that the relationship between the dependent variable and the independent variables is nonlinear, forcing the predicted values to range between 0 and 1. This avoids the problem of nonsensical predictions common to the OLS model, which may produce negative predicted values or values greater than one (Long 1997: 39). Second, the relationship between the dependent variable and key independent variables may not be linear. For instance, as I argue in the theoretical framework, it seems unrealistic to assume that the effect of regime duration on the probability of a nonviolent campaign onset is constant across different values of regime duration. Instead, the logistic model assumes an S-shaped relationship between the independent variables and the probability of an event, which allows the effects to vary over different levels of the independent variables (Long 1997: 39-40). The main difference between the OLS model and the logistic regression model thus lies in the assumption of the functional form of the true population regression function. The OLS model works from the assumption of a standard normal distribution function. In contrast, the logit model works from the assumption of a cumulative standard logistic distribution function, defined in terms of the exponential function:

$$\Pr(Y = 1 | X_i) = \frac{\exp(\beta_i X_i)}{1 + \exp(\beta_i X_i)}$$

where the probability of the event, Y = 1, given a set of coefficients and covariates, β_i and X_i, are attained by taking the exponentiation of the log odds divided by 1 plus the log odds (Long 1997: 51; Stock and Watson 2012: 429-436). The logistic regression model is therefore expressed in terms of *log odds* (or logit) rather than probabilities. A coefficient estimate

[33] Only three countries experienced both governmental and territorial campaigns in the same year: China (1989), Indonesia (1997) and Nigeria (1993-1995)

Figure 4.2 *Nonviolent campaigns around the world, 1945-2006.*



Note: World geography as of 2008.

should thus be interpreted as the expected change in the log odds of a nonviolent campaign onset, given a one-unit increase on the relevant independent variable, holding the other independent variables constant. However, aside from giving a general indication of the direction and strength of an effect log odds have no intuitive interpretation (Long 1997: 49). I therefore supplement regression tables with calculations of predicted probabilities to make the results more accessible.

Alternatively, one could also use the probit regression model, which address the same problems noted above. Because the two models tend to produce very similar results, choosing between one estimator and the other is usually a matter of convention (Stock and Watson 2012: 436).[34] In my case I have chosen to rely on logit regressions, as they are more commonly used within the literature on nonviolent conflict and political science in general. As the robustness checks in Chapter 5 shows (Tables D.1 and D.2 in Appendix D), the two estimators produce close to identical results.

Finally, because the statistical model is estimated on time-series cross-section (TSCS) data, ordinary logit and probit models are bound to be problematic. Both the temporal and spatial properties of TSCS data are likely to violate assumptions that error terms are uncorrelated and have the same variance; observations may be correlated across time (temporal autocorrelation) or geographical units (spatial autocorrelation), and the variances within different countries are unlikely to be identical (Stock and Watson 2012: 368-369). In OLS, autocorrelation and panel-level heteroskedasticity both lead to biased standard errors, but do not affect the parameter estimates. In the case of ordinary logit and probit models, however, heteroskedasticity is likely to generate *both* inconsistent standard errors *and* incorrect parameter estimates (Kennedy 2003: 268). To address potential autocorrelation and panel-level heteroskedasticity, I therefore implement the following measures. First, I rely on robust standard errors clustered on each country, which has been shown to have excellent properties in dealing with both heteroskedasticity and autocorrelation (e.g. Beck and Katz 1995).[35] To tackle temporal autocorrelation, I also include a lagged dependent variable. Spatial autocorrelation is further addressed by including two neighborhood variables in all the extensive models. Finally, I also include decade and region dummies in the most extensive models to control for additional temporal or regional trends.

[34] Both models rely on maximum likelihood estimation. The main difference the two estimators is that the probit model specifies a moderately different cumulative distribution function.

[35] Admittedly, Beck and Katz's (1995) article deal with "panel-corrected" standard errors rather than clustered robust standard errors. However, the properties of these two methods are by and large the same (King and Roberts 2014).

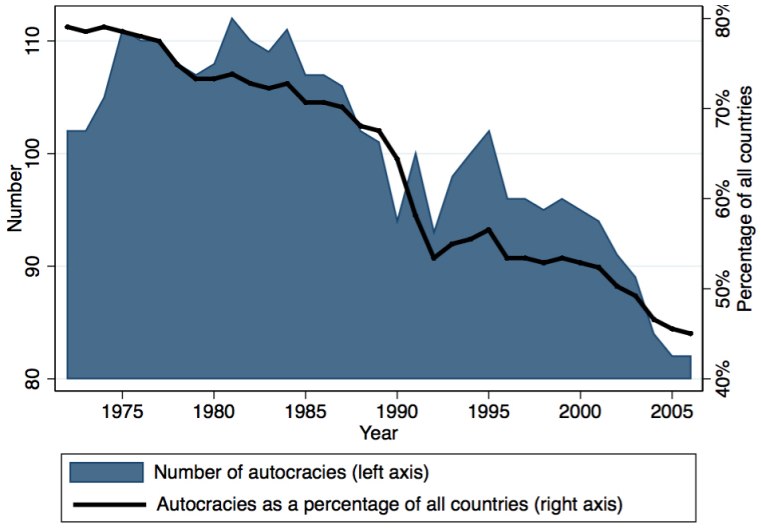
4.3 Operationalizations of the Independent Variables

This section looks at the choice of empirical indicators for testing the theoretical propositions in Chapter 3. Choosing indicators that best reflect the theoretical concepts under study is crucial with regards to measurement validity (Adcock and Collier 2001). Accordingly, I account for the choice of independent variables, and present the set of control variables included in the study.

4.3.1 Degree of institutionalization

As discussed in Chapter 1 and 2, no consensus exists on how to operationalize authoritarian regimes. No single index or categorization is withheld problems with reliability or validity (Cheibub et al. 2010; Munck and Verkuilen 2002). Ultimately, the choice of measure depends on the specific research question in mind, and evaluations of how latent variables can best be measured empirically (Adcock and Collier 2001). Here, I rely on a modified version of Hadenius and Teorell's (2007) original typology of authoritarian regimes to separate between different dictatorships (Wahman et al. 2013).

Figure 4.3 *The number of autocracies in the world, 1972-2006*



The authors first separate democracies and dictatorships by drawing a threshold of 7 on a 10-point scale created by taking the mean values of the Freedom House and Polity scores (both normalized to range between 0 and 10). All countries with a democracy score below 7 are

classified as authoritarian.[36] Subsequently, they classify authoritarian regimes according to three different modes of accessing and maintaining political power: (1) hereditary succession, or lineage, (2) the actual or threatened use of military force and (3) popular elections and related party structure. Figure 4.3 shows the frequency of autocracies in the world in the period under study.

This typology thus corresponds quite closely to the original typology introduced by Geddes (1999). However, Hadenius and Teorell (2007) make two distinctions, which are particularly important for my analysis. First, the authors separate party-based regimes in which only *one* party is permitted from party-based regimes where one or more opposition parties are allowed. This distinction is essential if we are to test the theoretical stipulations offered in the previous chapter. Second, unlike Geddes, Hadenius and Teorell do not treat personalist regimes as a separate category. Instead, they argue that personalism is a continuous trait more or less present in all regimes (Hadenius and Teorell 2007: 149). This avoids confounding effects of personalism with what is really due to a country's institutional infrastructure (Lai and Slater 2006: 115). Moreover, as Knutsen and Fjelde (2013: 101) note, "it is difficult to objectively operationalize and score personalism, particularly since the basis of power relations within the regime and control over political processes are inherently difficult to observe". Instead, the main categories in Hadenius and Teorell's classification rely on 'observable' formal institutional characteristics to a larger extent, which should improve reliability.

The focus on the institutional setup of regimes also has important merits in terms of content validity. A common approach in the literature has been to identify regimes based on the characteristics or social origins of ruling elites. Geddes et al. (2014), for instance, classify regimes according to the identity of the leadership groups rather than formal institutional characteristics. Similarly, Cheibub et al. (2010: 84) separate dictatorships according to the characteristics of "the inner sanctums where real decisions are made and potential rivals are kept under close scrutiny". By contrast, the classification in the Authoritarian Regimes Data Set is "based on the institutions on which [ruling] elites rely on in order to regulate the access to and maintenance of public authority" (Wahman et al. 2013: 21). Arguably, this makes Hadenius and Teorell's (2007) framework more suited to the current analysis than alternative conceptualizations. After all, the theoretical argument proposed in Chapter 4 relate primarily

[36] Note that the threshold for democracy has been lowered from 7.5 since the original edition of the Authoritarian Regimes Data Set (Hadenius and Teorell 2007).

to how different regimes maintain political control and how the degree of access to institutional venues affect the opposition's incentives for nonviolent action.

A final consideration that has guided the choice of Hadenius and Teorell's (2007) framework over alternative classifications is the requirement that electoral regimes hold regular popular elections for parliament or executive office. Cheibub et al. (2010), for instance, also separate regimes with one party, multiple parties or no parties. However, they do not demand that a regime holds regular elections to be classified as a multiparty regime. As specified in the theoretical framework, multiparty elections are one of the main features of that make multiparty autocracies less prone to nonviolent campaigns. In the absence of multiparty elections we should not expect the permission or existence of multiple parties to have such a dampening effect. Nevertheless, for the skeptical reader I will run robustness checks with Cheibub et al.'s (2010) categorizations. Using their data also allows me to test whether the results hold with a procedural definition of democracy.

Hadenius and Teorell's (2007) original classification thus separate six main categories of authoritarian regimes: monarchies, military regimes, no-party electoral regimes, single-party regimes, multiparty autocracies and other autocracies. One potential caveat with this approach is the danger of collapsing multiple, conceptually distinct dimensions of dictatorships onto a single typology. As Svolik (2012: 32) notes, such typologies tend to separate 'types' that are "neither mutually exclusive nor collectively exhaustive and often ask for difficult classification judgments that weigh incommensurable aspects of authoritarian politics". For instance, several military regimes hold both single-party and multiparty elections. In order to address this problem, I use two alternative dimensions from Wahman et al.'s (2013) data: one for the level of party institutionalization, and one for the level of military involvement in politics. The first dimension constitutes the main explanatory variable, and separates between three different levels of restrictions on political parties: a regime may either (0) formally or de facto ban political parties, (1) allow one party, or (2) allow several.[37] The second dimension controls for regimes with high levels of military involvement in politics (see elaboration in section 4.3.3). Both measures are lagged by one year to reduce endogeneity. Given the limited number of nonviolent campaigns overall, specifying separate dimensions also has clear advantages in terms of utilizing the available data. If the independent variable is too fine-grained, we risk problems with empty cells – a problem that may be particularly acute when applying a binary dependent variable. Fewer

[37] The two last categories are drawn from the *onep* and *mul* dummies in Wahman et al.'s (2013) dataset. Those dictatorships that scored 0 on both categories were used to construct the last category of *no-party* regimes.

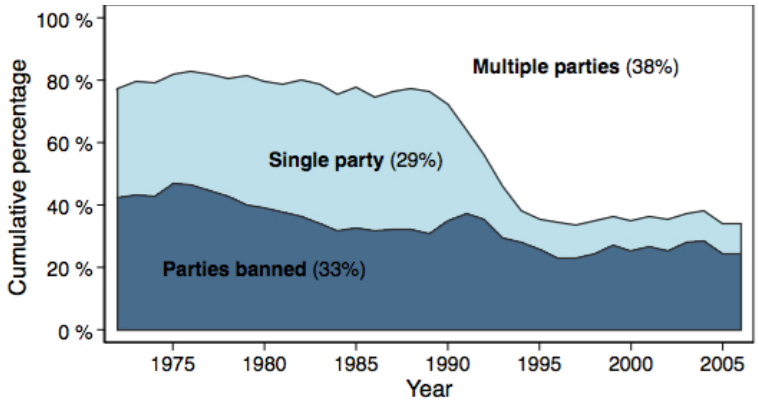
Table 4.1 *Degree of Institutionalization by Region, 1972-2006*

Region	Degree of Institutionalization			
	Parties banned	**Single party**	**Multiple parties**	**Total**
Sub-Saharan Africa	436 (12%)	451 (13%)	481 (14%)	1,368 (39%)
Middle East/ North Africa	356 (10%)	121 (3%)	149 (4%)	626 (18%)
Asia	250 (7%)	189 (5%)	294 (8%)	733 (21%)
Latin America/ Caribbean	125 (4%)	38 (1%)	221 (6%)	384 (11%)
Eastern Europe/ ex-U.S.S.R.	4 (<1%)	197 (6%)	176 (5%)	377 (11%)
Western Europe/ North America	5 (<1%)	5 (<1%)	8 (<1%)	18 (<1%)
Total	1,176 (33%)	1,001 (29%)	1,338 (38%)	3,515 (100%)

Notes: Listed by number of country-year observations. The region categories are based on Knutsen and Fjelde (2013).

categories ameliorate this problem, and also make sure that the data are more evenly distributed. As Table 4.1 and Figure 4.4 show, no-party regimes, single-party regimes and multiparty regimes are all quite widely distributed across space and time.

Figure 4.4 *Restrictions on Political Parties by Year*

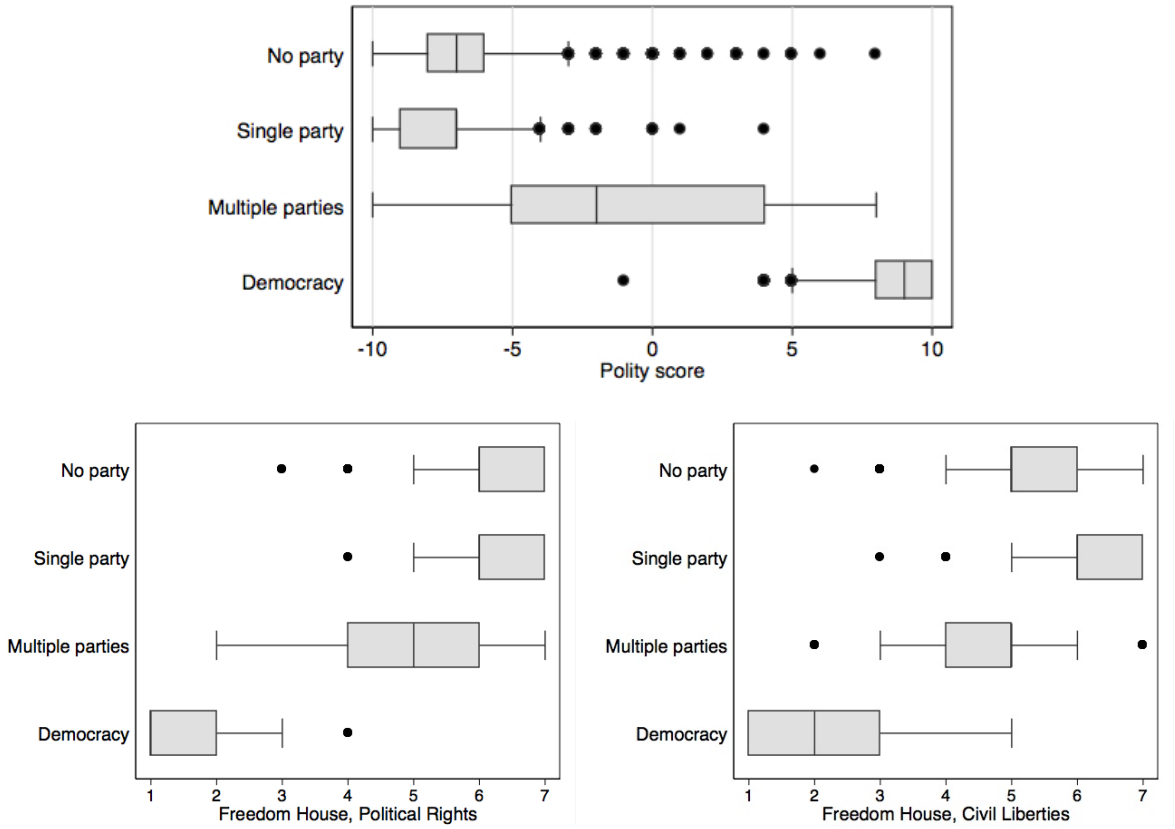


Notes: Overall distribution of individual categories in parentheses. Observations: country-years.

When combined with data from NAVCO 2.0, the new dataset covers 191 countries in the period between 1972 and 2006.[38] 143 of these countries were registered as authoritarian at one point or another. In total, 64% of the 5,943 country-year observations had an authoritarian government. Figure 4.5 compares the respective levels of institutionalization by Polity and Freedom House scores. No-party and single-party regimes tend to be more authoritarian and repressive, whereas multiparty autocracies tend to fall somewhere between democracies and the group of closed regimes on all dimensions. However, there is considerable variation within each category, especially among multiparty autocracies.

[38] The dataset starts in 1972 because their coding is dependent on data from the Freedom House Index, which are only available in the period 1972-2010.

Figure 4.5 *Degree of Institutionalization by Polity and Freedom House scores*



Note: The line inside the boxes represent median values, the edge of the boxes represent the 25th and 75th percentiles, the whiskers represent the lower and upper adjacent values, and the dots represent outliers.

4.3.2 Regime Duration

To test whether there are certain conditions that make multiparty autocracies more likely than other autocracies to experience nonviolent campaigns, I include a separate variable for *Regime duration*. To test for a conditional effect this variable is interacted with the level of restrictions on political parties. *Regime duration* counts the number of years since the last regime change or since independence. The variable is backdated to 1960. A regime change is recorded if a country experiences a transition from any of the seven original categories in Hadenius and Teorell's (2007) framework. It is possible that the effect of a marginal increase in regime duration is larger the shorter the brevity of a regime, as regimes tend to become more resilient to threats over time (e.g. Svolik 2009, 2012). I therefore follow Knutsen (2014), and specify the variable as *log of (regime duration +1)*$_{t-1}$ to focus on the marginal increase in regime duration. Figure A.1 in Appendix A compares the distribution of the untransformed and transformed regime duration variable for autocracies. Overall, the main

expectation is that the interaction between multiparty autocracy and proximity to regime change should be positive.

Admittedly, it is not given that a logarithmic transformation of the regime duration variable gives the best possible specification, although a concave function seems more appropriate than a linear one. An alternative operationalization would be to construct a decay function where the effect of regime duration is expected to decline exponentially over time (Fjelde 2010; Hegre et al. 2001). Thus, to make sure that the assumptions regarding the functional form of the variable do not bias the results I substitute the original variable with a decay function as an alternative specification (see Tables D.1 and D.2 in Appendix D). The results are robust to the choice of specification.

Note also that the short time coverage (1960-) on *Regime duration* is also potentially problematic, as it can conflate the risk of nonviolent uprisings in some of the longer-lasting regimes with dictatorships that have been less durable. However, because I am concerned with the effects of changes in the institutional structure of regimes, as opposed to the effects of leadership changes (e.g. Cheibub et al. 2010; Geddes et al. 2014; Svolik 2012), the current data arguably provide the best alternative.

Although *regime duration* is included in the conditional models, I do not include it as a control variable in the remaining models. The rationale is that including the variable may control for effects that are of substantive interest. Recall that the theoretical framework stipulates that regime duration can be a proxy for two different, but related, tendencies: political instability and authoritarian entrenchment. Some regimes may be particularly disposed to nonviolent campaigns in the early faces of their tenure, and it may seem reasonable to control for this effect. However, if other regimes experience a greater likelihood of nonviolent campaigns at later stages of their tenure, then it is ill-advised to control for this tendency as it may introduce post-treatment bias, i.e. controlling away the consequences of a variable (King and Zeng 2007).

4.3.3 Control variables

As noted previously, the present inquiry seeks to investigate whether we can discern any independent effect of political institutions on the likelihood of nonviolent campaigns in dictatorships. Thus, when seeking to explain the effect of an explanatory variable, *X*, on a dependent variable, *Y*, it is essential to control for the variable, *Z*, that potentially disturbs the association between *X* and *Y*. This is to avoid the problem of omitted variable bias, which

occurs when a variable that is correlated with both the dependent and the independent variable of interest is left out of the analysis (Stock and Watson 2012: 222). In this section, I present the control variables that I include in my main models.

I specify a baseline model, which includes only a restricted set of covariates that have commonly been used in previous studies of contentious politics: military regimes, GDP per capita, urbanization, natural resource revenues, population size, as well as a measure for temporal dependence. These controls also include some of the variables that are of key theoretical interest apart from the explanatory variables. A more restricted model obviously has clear advantages in terms of communicability. Furthermore, there is also the concern of adding variables that do not belong, which may reduce the precision of the coefficient estimates (Stock and Watson 2012: 358-359). Omitted variable bias always has to be weighed against the risk of collinearity (Schrodt 2014).

However, logistic regression models are particularly sensitive to omitted variable bias, even when these variables are unrelated to the independent variables in the model (Kennedy 2003: 268; Mood 2010). As Mood has shown, omitted variable bias in logistic regressions has the effect of giving coefficient estimates that are significantly biased towards zero.[39] Thus, the potential gains in efficiency from a more restricted model may come at the expense of committing type II errors, i.e. the failure to reject a false null hypothesis. In the remaining analyses I therefore estimate more extensive models specifying additional covariates that may be significantly confound the relationship between political institutions and nonviolent campaign onsets. I present these variables below.

Baseline Model

As emphasized previously, a particularly important control variable is the level of military involvement in politics. Crucially, a higher level of military involvement could disproportionally affect the likelihood of nonviolent campaigns in countries with different levels of restrictions on political parties. Unlike other no-party regimes, military regimes that ban political parties tend to emerge in conditions with a strong and polarized opposition (Magaloni 2008: 11-12; Svolik 2012: 10-11). Unlike other multiparty regimes, military regimes that allow opposition parties are more likely to rely on violent repression than cooptation (Fjelde 2010) and reserve large domains of authority from public influence (Magaloni 2008: 17). Thus, no-party regimes and multiparty autocracies with high levels of

[39] Note that these problems ”also apply to coefficients from probit and most other models using non-linear link functions” (Mood 2010: 80).

military involvement may run a much higher risk of nonviolent campaigns than their civilian counterparts. I therefore use the dummy variable *mil* from Wahman et al.'s (2013) dataset to control for military regimes. Figure A.2 in Appendix A shows the distribution of political parties by military involvement in politics.

In order to proxy opposition strength, I include three variables: GDP per capita, urbanization and natural resources. As argued in Chapter 4, it is important to control for important determinants of opposition strength if we are to parcel out independent effects of political institutions. Thus, first, I control for the effects of a country's income level by including Gleditsch's (2002) estimate of Real GDP per Capita. The variable is log-transformed and lagged by one year. Economic development has repeatedly been suggested as a source of opposition strength. Observing the high correlation between income and democracy, several proponents of modernization theory (e.g. Boix and Stokes 2003; Lipset 1959) have advanced the claim that economic development has a causal impact on the likelihood of a democratic transition. Similarly, Butcher and Svensson (2014) has found a strong and positive association between the level of manufacturing to GDP and the likelihood of nonviolent campaigns. Economic modernization may increase the relative strength of groups that are likely to participate in nonviolent campaigns, such as the urban middle class or organized labor (Ansell and Samuels 2010). Alternatively, higher income may alter the expected material benefits from different regime types (Boix 2003).

Second, I control for the level of urbanization in a country by including a one-year lagged measure on the share of people living in urban areas from the World Bank (2012). Nonviolent campaigns are often urban phenomena (Celestino and Gleditsch 2013). Dahl et al. (2014), for instance, show a strong relationship between the level of urbanization and the likelihood of nonviolent conflict. Studies of revolutions have also shown a similar relationship (e.g. Knutsen 2014). It is also unclear whether a higher income level should increase the risk of nonviolent campaign onset in the absence of urbanization (Dahl et al. 2014). Thus, both indicators of economic development should be included in the analysis.[40]

Third, I also control for income from natural resources by including a one-year lagged variable for the total value of oil and gas production from Ross (2013). As suggested by Gandhi and Przeworski (2006) and Gandhi (2008), countries rich on natural resources typically face a lower need for cooperation and weaker opposition, both factors that lead to a lower levels of institutionalization and impede popular mobilization. Others have also noted

[40] Here, I am not too concerned with the point estimates of these controls, and thus do not consider it a problem if the inclusion of both variables yields more uncertainty in the individual estimates of income and urbanization.

the high resilience of "rentier states" to popular mobilization (Bueno de Mesquita and Smith 2010; Diamond 2010). The distribution of rents from oil and gas production may also provide and alternative instrument of cooptation (Bueno de Mesquita et al. 2003). Because the original variable from Ross is highly skewed, I transform the variable to per capita values and test the natural log of the variable, similar to Wright (2008).

Finally, I control for two additional variables in the baseline model: population size and ongoing campaign. Population size is generally found to be a strong determinant of revolutions, and nonviolent campaigns in particular (Butcher and Svensson 2014; Chenoweth and Lewis 2013b; Chenoweth and Stephan 2011; Cunningham 2013). Data on population are drawn from the United Nations Statistics Division (2014). The variable is log-transformed to reduce the impact of very large populations. Furthermore, to facilitate a special focus on nonviolent campaign onsets in particular, I control for whether there was a nonviolent campaign in the previous three years, as nonviolent campaigns may exhibit some degree of path dependence. This approach is congenial to the approach taken by Cunningham (2013). Temporal dependence is a frequent issue when dealing with binary dependent variables in time-series cross-section data, and may lead to incorrect parameter estimates (Beck, Katz, and Tucker 1998). There are several alternative approaches to modeling time dependence (see e.g. Carter and Signorino 2010), but the solution chosen here seems to fit the data quite well: within the period covered no individual campaigns have recurred after an lapse of more than three years. According to the NAVCO 2.0 data, all of the 16 cases of recurrence after a break of more than three years are cases of new campaigns emerging rather than old ones reemerging. Recognizing that there may be some additional temporal effects associated with having had a previous campaign, I also run robustness checks with a log-transformed measure of time since last campaign and a decay-function of the proximity of nonviolent conflict for the extensive models. The findings remain robust to these alternative specifications (see Table D.1 and D.2 in Appendix D).

Extensive models

In the remaining models I also control for a range of other covariates that may, potentially, affect the onset of nonviolent campaigns. *First*, diffusion effects have repeatedly been argued to be a common source of democratization and nonviolent uprisings (e.g. Beissinger 2007; Bunce and Wolchik 2006; Celestino and Gleditsch 2013). Nonviolent campaigns tend to be geographically and temporally clustered (see Figure 5.1 and 5.2), notable examples being the regional diffusion of nonviolent campaigns in Eastern Europe in 1989, as well as the

uprisings in the Middle East and North Africa in 2010-2011. It has also been suggested that greater linkages with democratic states may make nonviolent uprisings more likely by increasing the opposition's support base for mobilization (Celestino and Gleditsch 2013). I therefore control for a country's proportion of democratic neighbors and whether a neighboring country had a nonviolent campaign. I created these variables by combining regime-type data with contiguity data from the CShapes dataset (Weidmann, Kuse, and Gleditsch 2010).[41] I consider all countries within 500 km of a state's borders as neighbors.[42] *Second*, most communist regimes received material and institutional support from either Moscow or Beijing, which may explain their resilience to nonviolent campaigns (Svolik 2012: 113). I therefore include a dummy variable for communist leaders from the Cheibub et al. (2010). *Third*, polarization may also heighten the likelihood of a nonviolent campaign by intensifying the contention between the ruling coalition and those excluded from power (Gandhi and Przeworski 2006; Svolik 2012). Alternatively, polarization may make it harder for the opposition to unite (Schedler 2002b). I therefore control for a country's ethnic and religious composition by including the measures on ethnic and religious fractionalization from Alesina et al. (2003).[43] *Fourth*, there is also the possibility that ongoing civil wars could decrease the likelihood of a nonviolent campaign. I therefore control for civil wars by using the data on violent campaigns from NAVCO 2.0 (Chenoweth and Lewis 2013b). Both the neighbor variables and the variables for communist leaders and civil war are lagged by one year.

An additional concern is whether to control for military capacity or not. Many single-party regimes maintain substantial armies (e.g. Fjelde 2010), and controlling for military capacity may make single-party regimes appear more prone to nonviolent campaigns than they really are. However, large military forces are also common in dominant party regimes, and are therefore not confined to single-party regimes (Svolik 2012: 165). Furthermore, it could be argued that the tendency for single-party regimes to have exceptionally large militaries is primarily due to the totalitarian nature of many communist single-party regimes. Data on military personnel from the Correlates of War project (Singer 1988), for instance, reveal a substantial difference between communist and non-communist single party regimes,

[41] I want to thank Marianne Dahl for helping me construct these variables.

[42] In the case of Islands with no neighbors within the specified range I use the regional proportion of democracies (adjusted to exclude a country's own campaigns) for the democratic neighbors variable and code the neighboring campaign variable as 0.

[43] The two variables denotes the probability that two randomly selected people from a given country will not share a certain characteristics, in this case either ethnicity or religion.

with the former on average boasting armies ten times as large as the latter. These data also show that multiparty autocracies tend to control somewhat larger armies than non-communist single-party regimes.[44] Given that communist regimes only make up 35% of the single-party regimes, it appears justified to run a separate model controlling for the number of military personnel. I therefore include a lagged and log-transformed measure of the number of military personnel from the National Material Capabilities Data version 4.0 by the Correlates of War Project (Singer 1988).

Finally, to control for additional temporal and regional effects I also run separate models including decade and region dummies. The region dummies are based on Knutsen and Fjelde's (2013) classification, and are categorized as follows: (1) Western Europe with North America plus Australia and New Zealand, (2) Eastern Europe and (ex-) Soviet Union, (3) sub-Saharan Africa, (4) Asia and the Pacific, (5) Middle East and North Africa (MENA), and (6) Latin America and the Caribbean. I leave the final category as the reference category. Table 4.3 and 5.4 provide descriptive statistics for all the variables included in the main models. Because the main regression models exclude democracies at t-1, the descriptives are restricted to autocracies only to be more informative.[45] I provide the same data for all regimes in Appendix A (see Table A.1 and A.2), as well as frequency tables of the region and decade dummies (see Table A.3 and A.4).

Table 4.2 *Descriptive statistics for continuous variables 1973-2006, autocracies only*

Variable	Mean	Std. Dev.	Min.	Max.	N	Missing
Ln Regime duration$_{t-1}$	2.188	0.973	0	3.850	3430	0
Ln GDP per capita$_{t-1}$	7.901	1.119	4.889	13.237	3362	68
Urban population$_{t-1}$	41.177	22.876	2.716	100	3265	165
Ln Natural resources$_{t-1}$	0.305	0.718	0	4.502	3178	252
Ln Population$_{t-1}$	8.764	1.781	4.149	14.100	3375	24
Democratic neighborhood$_{t-1}$	0.237	0.256	0	1	3408	22
Ethnic fractionalization	0.518	0.260	0	0.930	3251	179
Religious fractionalization	0.427	0.247	0.002	0.860	3274	156
Ln Military personnel$_{t-1}$	3.562	1.761	0	8.466	3328	102

[44] The respective means of communist and non-communist single party regimes were 822,000 military personnel and 82,000 military personnel. Multiparty autocracies had on average 112,000 military personnel.
[45] Democracies are included in the final robustness checks (see Appendix D).

Table 4.3 *Frequency tables for binary variables 1973-2006, autocracies only*

Variable	0	1	Total	Missing
Nonviolent campaign onset	3226	204	3430	0
No-party regime$_{t-1}$	2275	1155	3430	0
Single-party regime$_{t-1}$	2439	991	3430	0
Multiparty autocracy$_{-1}$	2146	1284	3430	0
Democracy$_{t-1}$	0	0	0	0
Military regime$_{t-1}$	2542	888	3430	0
Neighboring campaign $_{t-1}$	2511	898	3409	21
Communist $_{t-1}$	3062	361	3423	7
Civil war$_{t-1}$	2802	628	3430	0

4.4 Methodological challenges

Although the framework presented so far should be well suited for testing the hypothesized effects of political institutions on nonviolent campaign onsets, two potential sources of bias remain that merit particular attention. The first methodological challenge concerns the causal relationship between the explanatory variables and the dependent variable. The second challenge relates to problems with missing data. Both represent important threats to the validity of my results.

4.4.1 Endogeneity

One of the key threats to the validity of the present inquiry relates to the problem of establishing the causal relationship between the explanatory variables and the dependent variable. This problem is also referred to as the problem of endogeneity, which arises when an independent variable is correlated with the error term. The practical implication of endogeneity is that coefficient estimates become biased, and it becomes hard to determine the causal direction between two variables (Stock and Watson 2012: 461-462). The problem of endogeneity is a substantial issue when analyzing both the origin and effects of political institutions. Przeworski (2004) goes as far as suggesting that regime types are epiphenomena, and thus the conditions that foster their creation, rather than the regimes themselves, should be the subject of inquiry. Endogeneity is also likely to be prevalent when studying the effects of political institutions on nonviolent campaigns. First, there is the previously noted problem of omitted variable bias: findings on the association between political institutions and

nonviolent campaigns may be driven by the underlying influence of opposition strength. This should complicate any causal inferences regarding the effects of political institutions. Second, there is the problem of reverse causality: several transitions to multiparty autocracies have been direct consequences of nonviolent campaigns (e.g. Greece in 1974 and Poland in 1989). Not accounting for this tendency could lead one to conclude that multiparty autocracies are more prone to nonviolent campaigns than what they really are.

The ideal solution to the endogeneity problem would be to run a so-called instrumental variables regression, which is commonly used to determine whether a relationship between two variables is causal or not (see e.g. Miguel, Satyanath, and Sergenti 2004). However, this approach depends on identifying an instrument that is both correlated with the explanatory variable and exogenous to the dependent variable, which is genuinely hard to find (Stock and Watson 2012: 463). I therefore rely on two alternative measures to retain the exogeneity of the independent variables to nonviolent campaigns. First, I try to control for some of the most relevant sources of opposition strength, such as the level of economic development and natural resource revenue. This arguably provides more comparable conditions for testing whether there are independent effects of political institutions on nonviolent campaigns. Second, I lag all time-varying covariates by one year. This is a common way of addressing endogeneity, as it empirically delineates cause and effect (see e.g. Hadenius and Teorell 2005). Admittedly, these solutions do not eliminate the potential endogeneity problem altogether.[46] However, this is where the strength of the theoretical framework comes into play: if the association between political institutions and nonviolent campaigns is entirely endogeneous, then we should expect multiparty autocracies to have a higher likelihood of nonviolent campaigns than no-party and single-party regimes. Otherwise, we arguably have evidence of "the channeling effect" hypothesized in Chapter 4.

4.4.2 Missing data

A final issue applicable to the majority of quantitative research on authoritarian politics is problems with missing data. Missing data are pervasive, as dictatorships are both more liable to misrepresent or obstruct data collection, and often constitute the most difficult conditions to collect data (Honaker and King 2010). Sample selection bias can potentially be a grave

[46] For instance, lagging the covariates may not be sufficient if current values on both the dependent variable and the independent variables are highly autocorrelated with past values (Knutsen 2011: 168). Furthermore, it is practically inconceivable to control for all relevant variables related to opposition strength.

threat to causal inferences, as it typically lead to both inconsistent and biased estimates (Stock and Watson 2012: 360).

Here, I have taken careful measures to avoid missing data by identifying sources with comparatively wide coverage. Nevertheless, some of these indicators still lack data, particularly the variables on urbanization, oil and gas value, and fractionalization (see Table 5.2). Consequently, about 11% of the country-year observations are left out through listwise deletion in the main extensive models, including 14% of the country-years with nonviolent campaigns.[47] In general, this is a relatively modest amount of missing units compared to what has been common in quantitative work on authoritarian politics (e.g. Boix and Svolik 2013; Gandhi 2008; Svolik 2012). As such, missing data arguably pose only a moderate threat, and I therefore employ listwise deletion in the main analyses.

Still, although the level of missingness is relatively modest, there is no guarantee that listwise deletion will not bias the results. For listwise deletion to be appropriate, the researcher must assume that data are *missing completely at random* (MCAR); whenever it is possible to predict the probability that a cell is missing, this assumption is violated. This is often an overly optimistic assumption, particularly in panel data (King, Honaker, Joseph, and Scheve 2001: 50-53).[48] In my case, this assumption does not seem to hold. Data on individual countries are lacking altogether, and there seems to be a pattern in which countries are lacking data. As Table 4.3 shows, a considerably higher number of single-party regimes lack observations on one or several of the control variables (see also Table A.5 in the Appendix). These regimes have about 18% missing observations, compared to 8,5% and 8,6% in no-party and multiparty autocracies. Moreover, a *majority* of the campaign-years left out through listwise deletion took place in single-party regimes. As such, the remaining sample may be biased towards single-party regimes.

Table 4.4 *Missing observations by level of institutionalization*

	No-party regime	**Single-party regime**	**Multiparty autocracy**
Non-events	95 (94,9%)	161 (90,5%)	101 (91,8%)
Campaign-years	3 (3,2%)	17 (9,5%)	9 (8,2%)

Note: Listed by number of country-year observations.

[47] Listwise deletion involves removing entire country-year observations if one or more variables have missing data.

[48] Instead, multiple imputation relies on the less strict assumption that the data are *missing at random* (MAR), which means that a cell can be predicted by the observed data, but not (after controlling for these observations) the remaining missing data (King et al. 2001: 50-53).

To guard against potential bias, I therefore run additional tests where missing data have been replaced with predicted data. For this task, I rely on *Amelia II* software from Honaker and King (2010) to perform "multiple imputations" of the missing units.[49] The idea of this technique is to use the available information from the observed portions of the data to predict the empty cells. This is done multiple times in order to construct 'completed' data sets where the observed values are the same, but the missing values are filled in with different imputations (Honaker and King 2010: 561). Multiple imputation has the obvious advantage of generating more data, which lead to greater efficiency and potentially ameliorate bias. Moreover, the method relies on the less strict assumption that the data are *missing at random* (MAR), rather than MCAR. This basically means that a cell can be predicted by the observed data, but not (after controlling for these observations) the remaining missing data (King et al. 2001: 50-53). The assumption is impossible to test on the basis of the data alone, but becomes more credible if one includes more information in the imputation model (Honaker and King 2010: 564).

Here, I impute values for the following six control variables (all of which are continuous variables): Natural resource revenue, Urban population, Ethnic fractionalization, Religious Fractionalization, GDP per Capita, and Population. For the imputation process, I include all the variables from the main models, most of the variables from the robustness checks, as well as some additional variables that should aid imputation.[50] Following the recommendations of Honaker and King (2010) I also include leads and lags of the independent variables, as well as two polynomials of time interacted with each cross-sectional unit to capitalize on possible trends in time. Furthermore, I include a ridge prior at 5 percent to tackle convergence problems associated with high missingness and high correlations among the covariates.[51] Accordingly, I estimate five datasets, which are then collapsed into one. Summary statistics for the imputed variables are shown in Table 4.5. Figure A.3 in the Appendix also compares the densities of the imputed values to the distribution of the observed values for all observations. Overall, the distributions on the imputed variables are very similar to the distributions on the original variables. Still, the imputations of some of the missing values are

[49] These imputations were estimated in R.

[50] These variables included Alesina et al.'s (2003) measure on linguistic fractionalization, Fearon's (2003) measure of ethnic fractionalization, the Polity index by Marshall et al. (2014), a measure of the level of Manufacturing to GDP from the UN Statistics division, and three measures from the World Bank on the level of fuel exports, total land area, and the mortality rate for children under 5. All variables can be found in the Quality of Government's basic dataset (Dahlberg et al. 2015). Due to computational difficulties I have restricted the number of variables used for imputation to 50 variables (excluding year and GWNO code).

[51] This measure became necessary if the polynomials of time interacted with the cross-sectional units are to be included in the model (Honaker, King and Blackwell 2014: 23). Otherwise, the imputation model will not run.

quite different from the remaining sample. Yet, this is to be expected when different types of regimes are associated with different levels of missingness (Honaker, King and Blackwell 2014: 30).

Table 4.5 *Summary statistics of the imputed data, autocracies only*

Variable	Mean	Std. Dev.	Min.	Max.	N	Missing
Ln GDP per capita$_{t-1}$	7.901	1.119	4.889	13.237	3430	0
Urban population$_{t-1}$	41.532	22.622	2.716	100	3430	0
Ln Natural resources$_{t-1}$	0.309	0.695	-0.351	4.502	3430	0
Ln Population	8.768	1.776	4.149	14.100	3430	0
Ethnic fractionalization	0.513	0.256	0	0.930	3430	0
Religious fractionalization	0.426	0.243	-0.029	0.860	3430	0

As a final note of caution, it should be emphasized that multiple imputation is not necessarily a perfect remedy for missing data. First, if observations are systematically missing on several indicators for certain countries, then the available data may be too scarce to provide reliable estimates of the missing observations. A number of communist single-party regimes, for instance, lack data on numerous development indicators, and imputations are therefore less likely to hit the mark. Second, imputations obviously challenges the scientific principle of making causal inferences on the basis of real-world phenomena (King, Keohane, and Verba 1994: 219-220). Nevertheless, multiple imputation does provide a valuable asset for assessing the validity of inferences from the observed data.

4.5 Summary

In this chapter I have presented the properties of the quantitative research design used to test the theoretical stipulations in Chapter 3. The purpose of the chapter has been to bridge the gap between the theoretical framework and the empirical analysis. I have argued that relying on logistic regressions of a binary dependent variable provides the best way of testing my hypotheses. These analyses will be run on a dataset covering 3,515 country-year observations with authoritarian regimes in the period between 1973 and 2006. Among these observations, 205 were country-years with ongoing nonviolent campaigns. The next chapter therefore proceeds with the empirical results.

5 Results

In this chapter I present the results from the empirical analysis. The theoretical framework suggested three hypotheses related to the impact of political institutions on nonviolent uprisings. First, I expect single-party regimes to have a higher probability of nonviolent campaign onset than both no-party and multiparty autocracies. Second, I suggest that multiparty autocracies run no higher risk of nonviolent conflict than no-party regimes. Third, I expect the effect of regime duration to be higher in multiparty autocracies than in other dictatorships. This chapter will test each of these propositions.

The empirical analysis will proceed in three stages. First, I start with a simple descriptive analysis of the data. Next, I present the multivariate regression results from the conditional and the unconditional models. Finally, I assess the empirical fit and robustness of the main models.

The findings offer support for all three hypotheses, albeit primarily in the case of governmental campaigns. I find that single-party regimes experience a higher likelihood of nonviolent campaign onset than both no-party and multiparty autocracies, and the differences in probability appear to be quite substantial. In particular, the difference between permitting one party and permitting several parties is found to be especially robust. I also find that no-party and multiparty autocracies experience virtually the same probability of nonviolent conflict, a pattern that remains practically unchanged throughout all of the model specifications. Finally, I find that the impact of restrictions on political parties is highly conditioned on the proximity to regime change. Whereas the probability of campaign onset decreases over time in no-party regimes, and remains relatively stable in single-party regimes, multiparty autocracies experience a much higher risk of onset over time.

5.1 Descriptive Statistics

Before embarking on the main analyses, it is useful to have a preliminary look at the data. Simple plots and cross-tabulations are necessary to get a good grasp of the data, and may also help in avoiding inferential errors (Achen 2005: 338). First, I consider the bivariate association between nonviolent campaign onsets and the level of restrictions on political parties. Table 5.1 displays the frequency of nonviolent campaign onsets for the three main categories of authoritarian regimes under study in the period between 1973 and 2006. At first

glance, the data seem to give only weak support for the idea that single-party regimes are more prone to nonviolent uprisings. Multiparty autocracies actually have the highest frequency of nonviolent campaigns overall, with 7,4% of country-year observations featuring nonviolent campaigns. This is somewhat higher number of campaigns than in single-party regimes (6,8%) and a considerably higher number than in no-party regimes (3,7%). Commensurate with previous findings, the table also shows that the vast majority of nonviolent campaigns took place in authoritarian regimes (86,5%).

Table 5.1 *Frequency of nonviolent campaign onsets by degree of party institutionalization*

	Country-year observations	Percentage of observations	Number of campaign onsets	Onsets/Country year obs.
No-party	1,155	20.08	43 (11)	0.0372 (0.0172)
Single-party	991	17.23	67 (61)	0.0676 (0.0769)
Multiparty	1,284	22.32	95 (68)	0.0740 (0.0583)
Non-autocracy	2,322	40.37	32	0.0134

Notes: Figures in parentheses denote the respective frequencies and fractions when military regimes are designated as an independent category. All variables are lagged by one year.

Yet, these figures can be somewhat misleading. Table 5.2 compares the total number of nonviolent campaigns in military and civilian regimes. It shows that military regimes have a noticeably higher frequency of nonviolent campaign onsets (7,3%) than civilian dictatorships (5,5%). As such, once we take into account the level of military involvement in politics, a different pattern is revealed for the main regime categories (see figures in parentheses in Table 5.1). Among civilian dictatorships, single-party autocracies feature the highest frequency of nonviolent campaigns, followed by multiparty autocracies and no-party regimes. Furthermore, as can be seen from Figure 5.1, this pattern is particularly pronounced for governmental campaigns.

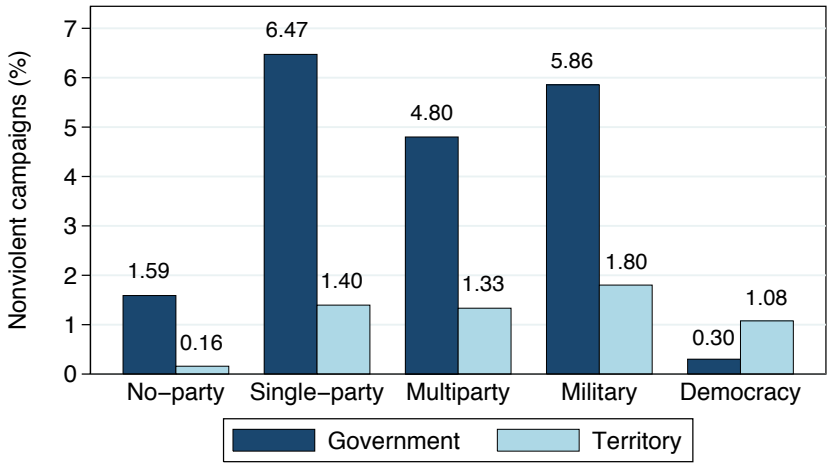
Table 5.2 *Frequency of nonviolent campaign onsets by military involvement in politics*

	Country-year observations	Percentage of observations	Number of campaign onsets	Onsets/Country year obs.
Military	888	15.44	65	0.0732
Civilian/Other	2,542	44.19	140	0.0551
Non-autocracy	2,322	40.37	32	0.0134

Overall, Hypothesis 1 (*H1*) therefore seems to receive some preliminary support; both multiparty autocracies and no-party autocracies appear to experience fewer nonviolent uprisings than single-party regimes. On the other hand, the descriptive statistics offer little

support for Hypothesis 2 (*H2*). In all cases, dictatorships with no parties have a considerably lower frequency of nonviolent campaigns than multiparty autocracies, although the ratio of governmental campaigns to territorial campaigns is much higher in these regimes than in multiparty autocracies. Obviously these figures display correlations rather than causations, and therefore have to be treated with some caution; the associations may not hold once we apply more sophisticated statistical models and control for potentially confounding variables, such as the level of rents derived from natural resources.

Figure 5.1 *Nonviolent campaigns by incompatibility and regime type*

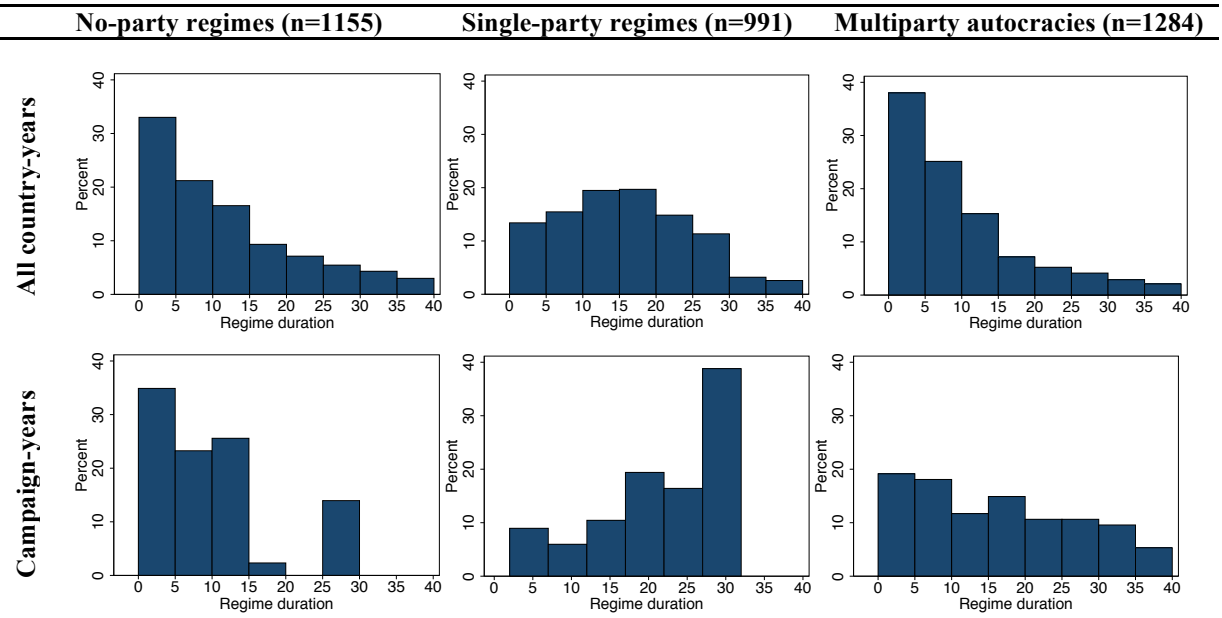


Finally, Hypothesis 3 (*H3*) postulates a positive interaction between regime duration and multiparty autocracies. The impact of multiparty autocracies relative to other autocracies should be conditioned on the duration of regimes. Figure 5.2 displays a pattern that is partially compatible with this assertion. The figure presents six histograms of the duration of authoritarian regimes, comparing the timing of nonviolent campaign onsets for no-party, single-party and multiparty autocracies. The top row shows the distribution of all country-years, while the bottom row displays only country-years with ongoing campaigns. Each column corresponds to the three different regime types. By comparing the relative distribution of campaigns by regime duration to the overall distribution of country-years one gets an idea of (1) the relative timing of onsets, and (2) the relative frequency of campaigns in a given period. I have restricted all histograms to a maximum duration of 40 years, as there are very few autocracies registered outside this interval (about 1,3% of autocracies). As can be seen from row three, a very high proportion of nonviolent campaigns in multiparty autocracies occur in later stages of a regime's tenure. In fact, about 59% of the nonviolent campaigns in multiparty autocracies occurred in regimes that had lasted more than ten years,

even though only 27% of all multiparty autocracies lasted that long. In comparison, 67% of nonviolent campaigns in no-party autocracies occurred within ten years of a regime change.

Yet, Figure 5.2 also shows that a large number of single-party regimes experienced nonviolent campaigns at a relatively mature stage. In part, this is due to the wave of nonviolent uprisings that took place at the end of the Cold War. For instance, in the period between 1989 and 1991, a total of 15 single-party regimes experienced nonviolent campaigns. The comparative figure for multiparty autocracies is seven countries. Moreover, one also has to take into account that the proportion of long-standing single-party regimes is much higher than the proportion of old multiparty autocracies. Although 72% of nonviolent campaigns in single-party autocracies occurred in regimes that had lasted more than ten years, this category contains 50% of all single-party regimes. Clearly this calls for further testing. In the next section I therefore proceed with the results from the main analyses.

Figure 5.2 *Distribution of nonviolent campaigns by regime duration, 1973-2006.*



Note: The row with "Campaign-years" displays the distribution of country-years recorded with non-violent campaigns. The number of campaign-years in each distribution is 43 (no-party regimes), 57 (single-party regimes), and 95 (multiparty autocracies).

5.2 Multivariate Regression Results

This section presents the results from the main regression models, testing the association between restriction on political parties and the onset of nonviolent campaigns in authoritarian regimes. I start off by considering the two unconditional hypotheses, *H1* and *H2*, which relate to the general likelihood of nonviolent campaigns in autocracies with different levels of

institutionalization. Next, I move on the conditional model, where I interact regime type with regime duration to test hypothesis *H3*. As explained in Chapter 4, all estimates are based on logistic regression models of authoritarian regimes between 1973 and 2006. Moreover, all time-varying covariates are lagged by one year to maintain some overall exogeneity from the dependent variable. Finally, all regression models are estimated in STATA 13.1.

5.2.1 Institutions and the Onset of Nonviolent Campaigns

Table 5.3 and 5.4 presents the results from the unconditional models. The first two "baseline models" consider the effect of institutions with only a restricted list of covariates. These covariates include the three explanatory variables, as well as six control variables, namely *military regime*, *GDP per capita*, *oil and gas value*, *urban population*, *population*, and *previous campaign*. The last three "extensive models" include an additional set of control variables. Table 5.3 reports the results when *single party* is set as the reference category, and Table 5.4 summarizes the main results with alternating reference categories.

In Model 1 I run the baseline model for all nonviolent campaigns, both governmental and territorial. The theoretical framework suggests that autocracies with one party should be significantly more likely to experience nonviolent campaign onsets than both no-party and multiparty autocracies. This expectation finds only limited support in the first model. On the one hand, the coefficients do show the expected signs: both the coefficients for *multiple parties* and *no parties* are negative when single-party regimes are set as the reference category. Yet, with respective p-values of 0.17 and 0.14, none of the associations are significant at conventional levels. The level of uncertainty surrounding the estimates is simply too high to provide assurance that they are statistically different from zero. Moreover, comparing the effects of *single party* with the effects of *military regime* also suggests that other institutional traits may be more important.[52]

On the other hand, all of unconditional models support the notion that multiparty autocracies face a comparable risk of onset to no-party regimes, and thus seem to confirm *H2*. When multiparty autocracies are specified as the reference category, the coefficient for *no parties* is close to zero and far from significant throughout all of the models. Moreover, the models provide no consistent estimate of the direction of the coefficient. Overall, the first model therefore seems to lend credence to Gandhi and Przeworski (2006) and Gandhi's

[52] The regime variables do seem to have a stronger effect than for instance *Population size*, but this is simply because the former are dichotomous measures while the latter is continuous. Unless two measures specify the same range, the sizes of the coefficients are not going to be directly comparable.

(2008) hypothesis that dictators are able to respond with the appropriate level of institutionalization to level dissent. Crucially, however, the remaining models do not.

Table 5.3 *Dictatorial institutions and the onset of nonviolent campaigns*

	Single party ref. category				
	(1)	***(2)***	***(3)***	***(4)***	***(5)***
No parties$_{t-1}$	-0.517	-0.580	-0.966**	-1.032***	-1.229***
	(0.380)	(0.388)	(0.398)	(0.391)	(0.407)
Multiple parties$_{t-1}$	-0.445	-0.610*	-0.971**	-1.064***	-1.146***
	(0.302)	(0.314)	(0.404)	(0.396)	(0.414)
Controls					
Military regime$_{t-1}$	0.605**	0.605**	0.590*	0.769**	0.528*
	(0.249)	(0.286)	(0.308)	(0.311)	(0.293)
Ln GDP per capita$_{t-1}$	0.305	0.313	0.130	0.340	0.212
	(0.228)	(0.234)	(0.222)	(0.226)	(0.305)
Ln oil and gas value$_{t-1}$	-1.342***	-1.374***	-1.692***	-1.743***	-1.534*
	(0.408)	(0.438)	(0.610)	(0.651)	(0.834)
Urban pop.$_{t-1}$	0.014	0.017*	0.022**	0.0282***	0.011
	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
Population$_{t-1}$	0.306***	0.213**	0.370***	0.836***	0.464***
	(0.105)	(0.102)	(0.099)	(0.164)	(0.106)
Democratic neighbors$_{t-1}$			0.666	0.478	-0.329
			(0.572)	(0.592)	(0.662)
Neighboring campaign$_{t-1}$			0.612**	0.672**	0.607**
			(0.279)	(0.285)	(0.280)
Ethnic fractionalization			0.568	0.180	0.176
			(0.610)	(0.585)	(0.634)
Religious fractionalization			-1.182**	-1.473***	-1.560***
			(0.562)	(0.534)	(0.554)
Communist$_{t-1}$			-0.747	-0.317	-0.882
			(0.557)	(0.586)	(0.661)
Civil war$_{t-1}$			-0.736**	-0.656**	-0.600*
			(0.318)	(0.317)	(0.314)
Previous campaign	3.534***	3.552***	3.433***	3.392***	3.362***
	(0.247)	(0.262)	(0.250)	(0.249)	(0.266)
Ln military personnel$_{t-1}$				-0.559***	
				(0.152)	
Intercept	-9.649***	-9.197***	-9.514***	-13.40***	-8.590***
	(1.589)	(1.643)	(2.114)	(2.367)	(2.390)
Log-likelihood	-418.73	-381.64	-361.03	-353.61	-348.24
AIC	855.45	781.28	752.07	739.21	742.48
Observations	3,095	3,066	3,010	2,959	3,010
Countries	128	128	127	127	127
Region dummies	no	no	no	no	yes
Decade dummies	no	no	no	no	yes

Notes: All models are logistic regression models. Model 1 features all nonviolent campaigns, while the remaining models exclude territorial campaigns. Data are from 1973-2006 in all models. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5.4 *Summary of main results: Dictatorial institutions and the onset of nonviolent campaigns*

Restrictions on Party Organization	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Single-party vs. multiple parties	0.445	0.610*	0.971**	1.064***	1.146***
	(0.302)	(0.314)	(0.404)	(0.396)	(0.414)
Single party vs. no party	0.517	0.580	0.966**	1.032***	1.229***
	(0.380)	(0.388)	(0.398)	(0.391)	(0.407)
Multiple parties vs. no party	0.072	-0.030	-0.004	-0.032	0.083
	(0.377)	(0.412)	(0.415)	(0.409)	(0.414)

Notes: Presents the same results as in Table 6.3, only with alternating reference categories. Estimates for the control variables are not reported because they are identical to the ones in Table 6.3. Data are from 1973-2006 in all models. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Model 2 runs the same baseline model, but excludes the 29 country-years with only territorial campaigns. As can be seen from Table 5.3, both the terms for *No parties* and *Multiple parties* show stronger effects, particularly the latter. Moreover, with a p-value of 0.052, the estimate for *Multiple parties* is now significant at the 10%-level and very close to being significant at the 5%-level. Although the estimate for *No parties* still fails to reach significance at conventional levels, the results nevertheless indicate that even a simple baseline model diverges considerably from Gandhi and Przeworski's (2006) expectations. The results show that single-party regimes are indeed more prone to governmental campaigns, particularly when compared to multiparty autocracies. The effect of allowing only one party is now comparable to having a military regime. Surely the results are still associated with a considerable level of uncertainty. Yet, moving on to the extensive models suggests that this uncertainty is by and large the product of an underspecified model.

Model 3 presents the full unconditional model, including all the main control variables. The results offer clear support for *H3*: single-party regimes are significantly more likely than both no-party regimes and multiparty autocracies to experience nonviolent campaign onsets (both estimates are close to being significant at the 1%-level). The impact of having a single party is also comparatively strong. For instance, it is greater than the effects of *neighboring campaign(s)*, which has commonly been argued to be a powerful determinant of nonviolent campaign onset (e.g. Beissinger 2007; Bunce and Wolchik 2006).[53] Furthermore, the results also suggest that the dampening effect of having multiple parties is just as strong as the conflict inducing effect of having a single party. Otherwise, we would expect multiparty autocracies to have a higher risk of onset than both single-party and no-party regimes (Gandhi 2008; Gandhi and Przeworski 2006).

[53] This association also hold if we drop the one-year lag on *neighboring campaign* (not displayed).

Because repressing the opposition is commonly suggested as an alternative to co-opting them (e.g. Bueno de Mesquita and Smith 2010; Wintrobe 1998), it is useful to consider the differences between regime types when their repressive capacity is held constant. Model 4 therefore tests the effect of controlling for the number of *military personnel*, which can be used as a proxy for for repressive capacity. The results in column 4 in Table 5.3 show that larger militaries have a very significant effect in terms of deterring nonviolent mobilization. Moreover, controlling for military personnel also leads to a somewhat stronger and more significant difference between single-party regimes and the other two regime categories. Arguably, this is indicative of a stronger institutional effect of multiparty elections than what can be observed from the raw data. The reason why we see a greater difference between single-party regimes and no-party regimes is probably because party regimes typically maintain larger armies (Svolik 2012: 165). However, because there is some discussion as to whether large repressive apparatuses are constitutive parts of single-party regimes or not (see e.g. Magaloni and Kricheli 2010; Svolik 2012), I refrain from including *military personnel* in the remaining analyses. Otherwise, I risk introducing post-treatment bias. Finally, including region and decade dummies in the model only substantiate the findings in Model 3. The estimates for *no parties* and *multiple parties* in Model 5 display even stronger effects and are now both significant at the 1% level. In fact, once we control for region-specific effects and temporal trends, Model 2 also proves highly supportive of *H1* (see Table B.1 in Appendix B for the complete models).

In order to better gauge the substantive impact of these effects, Table 5.5 and Figure 5.3 presents changes in predicted probabilities calculated from Model 3.[54] The table displays how we should expect the risk of campaign onset to vary when each independent variable is allowed to vary from its 10^{th} to 90^{th} percentile for continuous variables and from 0 to 1 for dichotomous variables while holding all other variables constant at their mean or median. Figure 5.3 provides visual representation of these changes along with measures of uncertainty. The diamonds in the figure show the raw change in the risk of campaign onset as a state moves between percentiles for a given variable, while the whiskers represent 90% confidence intervals.

First, we see that the likelihood of campaign onset varies considerably from single-party to no-party and multiparty autocracies. While no-party regimes and multiparty autocracies both have a 0.8% annual predicted probability of a campaign onset, the probability of onset in

[54] I used *Clarify* from King, Tomz, and Wittenberg (2000) to obtain these predictions.

Table 5.5 *Predicted probabilities of nonviolent campaign onset*

	10th percentile	*90th percentile*	*%Change*	*ΔP*
No-party regime$_{t-1}$	0.80	0.80	0.7%	1.01
Single-party regime$_{t-1}$	0.80	2.10	161.9%	2.62
Military regime$_{t-1}$	0.80	1.50	86.6%	1.87
Ln GDP per capita$_{t-1}$	0.71	1.02	43.4%	1.43
Ln urban population$_{t-1}$	0.73	2.76	267.4%	3.67
Oil and gas value$_{t-1}$	1.19	0.19	-83.6%	6.10
Population$_{t-1}$	0.37	1.60	332.7%	4.33
Democratic neighbors$_{t-1}$	0.71	1.02	44.4%	1.44
Neighboring campaign$_{t-1}$	0.80	1.48	84.1%	1.84
Ethnic fractionalization	0.68	0.94	38.4%	1.38
Religious fractionalization	1.24	0.57	-54.3%	2.19
Communist$_{t-1}$	0.80	0.35	-42.8%	1.75
Civil war$_{t-1}$	0.80	0.40	-50.0%	2.00

Notes: The table shows the predicted probabilities of a nonviolent campaign onset for the 10th and 90th percentile values of the given variable, all other covariates held at their mean (continuous measures) or modal values (dichotomous measures). When calculating the respective scores for no-party and single-party regimes the other party restrictions variables were held at zero. *%Change* refers to the relative change between the 10th and 90th percentile. *ΔP* denotes the ratio of the highest predicted probability to the lowest for each variable. All probabilities have been multiplied with 100 to ease interpretation. Estimations are based on Table 6.2, Model 3.

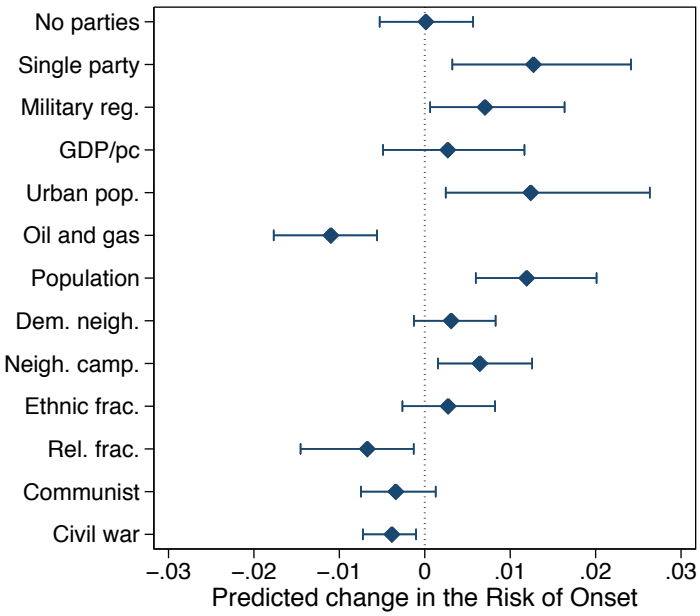
single-party regimes is 2.1%. Thus, moving from the multiparty or no-party autocracy category to the single-party autocracy category almost triples the likelihood of nonviolent conflict. The overall low baseline probabilities reflect that nonviolent campaigns are rare events. This is similar to other analyses of rare dependent variables, such as civil wars and coup d'états.[55] The relative increase in risk associated with a move to single-party autocracy is nevertheless quite substantial. Furthermore, this is most likely a conservative estimate. For instance, basing the predictions on Model 5 suggests that single-party regimes are 3.1 times more likely than multiparty autocracies to experience a campaign onset. In sum, these results provide strong support for both *H1* and *H2*.

Second, we also see that the impact of institutions is larger than the majority of the control variables. The only variables that have a larger effect than restrictions on political parties are *Urban population*, *Oil and gas value*, and *Population*. The fact that these covariates display such large effects is generally consistent with previous work and my theoretical expectations. However, note that the magnitude of these effects is to a large extent a product of comparing countries with low and high values on the respective variables, and to a lesser extent due to the dynamic processes of urbanization, increases in natural resource

[55] King and Zeng (2001), for instance, argue that logistic regressions with binary dependent variables tend to underestimate the probabilities of events in the case of rare events data. Thus, the current estimates are likely to be too small.

revenue and population growth.[56] Arguably, this suggests that the influence of institutions on the risk of nonviolent uprisings may be even larger than what can be read from comparing these numbers alone.

Figure 5.3 *The substantive impact of institutions on the onset of nonviolent campaigns*



Notes: Values show first difference (FD) estimations (◆) with 90% confidence intervals (|---|). Estimations are based on Table 6.2, Model 3.

5.2.2 Conditional effects: Institutions and Regime Duration

Table 5.6 tests the conditional hypothesis *H3*, which stipulates that the effects of restrictions on political parties depend on the duration of a regime. In particular, I expect the interaction between multiparty autocracy and regime duration to be positive when multiparty autocracies are compared to autocracies that ban opposition parties. To investigate this assumption I present three models, all of which are extensions of Model 3 in Tables 5.3 and 5.4. The first model adds only *Ln Regime duration* to the full model, while the last two models include one or several interaction terms.

Model 6 tests the consequences of including *Ln Regime duration* in an unconditional model. As can be seen from column one in Table 5.6, the term for *Ln Regime duration* is

[56] In fact, a change from the 10th percentile to the 90th percentile is hardly realistic in any of these cases, and may overestimate the impact of these variables. For instance, the increase in risk associated with a move to single-party autocracy is comparable to the risk associated with a move from the 20th to the 85th percentile on *Population* and a move from the 20th to the 82nd percentile on *Urban population* (the ratio (ΔP) between the lowest and the highest predicted probability in these two examples were 2,63 for *Population* and 2,64 for *Urban population*). Yet, no comparable leaps in population size or the level of urbanization have happened in the period under study.

positive and significant at the 5%-level, indicating that nonviolent uprisings becomes more likely over time. We also see that including *Regime duration* in the regression model both reduces the coefficient size for *Single party* and causes the term to lose significance (p-value of 0.12) when *Multiple parties* is specified as the reference category. Without further inquiry, these results could be interpreted as proof that nonviolent campaign onsets are less likely under conditions of political instability. Following this line of reasoning would suggest multiparty autocracies have a lower probability of nonviolent conflict partly because they are more disposed to political instability. However, if the theoretical stipulations in Chapter 3 are correct these results should instead be due to the different effects of regime duration on the risk of nonviolent conflict in multiparty autocracies, as compared to other autocracies.

Model 7 runs a first test of this argument, investigating whether we can discern any differences in the effects of regime duration between the three main regime categories. The idea is to examine to what extent the impact of regime duration in multiparty autocracies is different from the impact of regime duration in no-party and single-party regimes. If multiparty autocracies do not display a distinct trend, it may be inappropriate to compare multiparty autocracies against a baseline that includes both no-party and single-party regimes. Accordingly, Model 7 specifies *Multiple parties* as the reference category, and adds two interactions: one between *No parties* and *Regime duration*, and another between *Single party* and *regime duration*. The coefficient for the raw variable, *Ln Regime duration*, can now be read as the effect of regime duration on the likelihood of a campaign onset in multiparty autocracies. To interpret the interaction terms, we add the coefficient for the interaction term to the coefficient for the raw variable. For instance, the effect of *Regime duration* in no-party regimes is $\beta_{Ln\ Regime\ duration} + \beta_{No\ parties*regime\ duration}$. The constitutive terms for *No parties* and *Single party* should be read as the effect of these variables, as compared to *Multiple parties*, when *Ln Regime duration* is zero.

As we can see, the coefficient for *Ln Regime duration* in model 7 is highly significant (1%-level) and more than twice the size of the coefficient for *Regime duration* in Model 6, indicating a strong effect of regime duration in multiparty autocracies. In comparison, the coefficient for *No parties*regime duration* is negative, strongly significant, and greater in absolute value than *Ln Regime duration*, which implies that the probability of a nonviolent campaign onset decreases over time in no-party regimes. The coefficient for *Single party*regime duration* is also negative, but is smaller and not quite statistically significant (p-value of 0.13). This indicates that single party regimes become more likely to experience

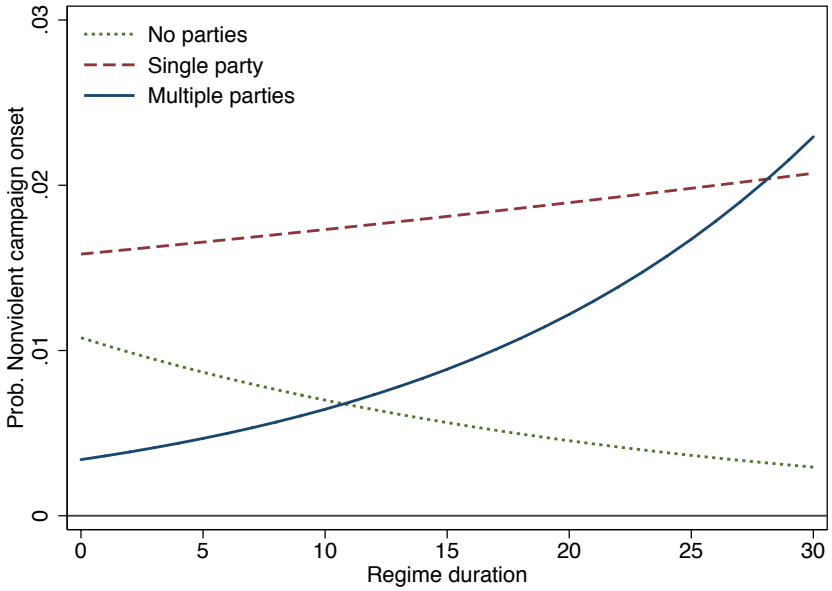
nonviolent conflict over time, but that this increase is much smaller than in multiparty autocracies.

Table 5.6 *Interaction between political party restrictions and regime duration*

	(6)	***(7)***	***(8)***
No parties$_{t-1}$	0.074	2.668***	-1.018***
	(0.417)	(0.774)	(0.382)
Single party$_{t-1}$	0.669	1.612	*Ref. cat.*
	(0.424)	(1.069)	
Multiple parties$_{t-1}$	*Ref. cat.*	*Ref. cat.*	-3.340***
			(0.758)
Ln Regime duration$_{t-1}$	0.424**	0.936***	-0.111
	(0.184)	(0.265)	(0.192)
*No parties*regime duration*		-1.240***	
		(0.332)	
*Single party*regime duration*		-0.427	
		(0.404)	
*Multiple parties*regime duration*			1.050***
			(0.301)
Controls			
Military regime$_{t-1}$	0.466	0.472	0.505
	(0.308)	(0.312)	(0.309)
Ln GDP per capita$_{t-1}$	0.111	-0.017	-0.011
	(0.219)	(0.241)	(0.238)
Ln oil and gas income$_{t-1}$	-1.663***	-1.494**	-1.443**
	(0.587)	(0.629)	(0.607)
Urban population$_{t-1}$	0.018*	0.0192*	0.019*
	(0.011)	(0.011)	(0.011)
Population$_{t-1}$	0.315***	0.306***	0.321***
	(0.097)	(0.104)	(0.103)
Democratic neighbors$_{t-1}$	0.669	0.858	0.873
	(0.609)	(0.631)	(0.629)
Neighboring campaign$_{t-1}$	0.642**	0.602**	0.693**
	(0.279)	(0.266)	(0.280)
Ethnic fractionalization	0.527	0.417	0.375
	(0.601)	(0.564)	(0.563)
Religious fractionalization	-1.094*	-0.932	-0.899
	(0.561)	(0.581)	(0.578)
Communist$_{t-1}$	-0.856	-0.830	-0.636
	(0.565)	(0.585)	(0.575)
Civil war$_{t-1}$	-0.752**	-0.597*	-0.624*
	(0.340)	(0.310)	(0.321)
Previous campaign	3.511***	3.553***	3.554***
	(0.262)	(0.256)	(0.258)
Intercept	-9.584***	-9.864***	-6.760***
	(1.956)	(1.978)	(1.944)
Log-likelihood	-354.72	-343.85	-345.82
AIC	741.44	723.69	725.63
Observations	3,010	3,010	3,010
Countries	127	127	127

Notes: All models are logistic regression models. Territorial campaigns are excluded and none of the models include region or decade dummies. Data are from 1973-2006 in all models. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 5.4 *Conditional effects of restrictions on political parties given regime duration*



Notes: The graph shows the conditional marginal effects of restrictions on political parties with all other covariates held at their mean (continuous variables) or modal values (dichotomous variables).

Figure 5.4 displays these effects graphically. The figure shows how the predicted probabilities vary over time for the three main regime categories when all of the control variables are held constant at their means (for continuous measures) and modes (for dichotomous measures).[57] To facilitate reading, I use the untransformed *Regime duration* variable.[58] The graph lends clear support to the idea that the impact of regime duration is different in dictatorships with multiple parties. In these regimes, the risk of a campaign onset is very low following regime change, increases substantially over time, and even approaches the risk of onset in single-party regimes. In comparison, single-party regimes display a much flatter curve, while no-party regimes show a quite substantial negative trend. In sum, Figure 5.4 seems to refute the idea that nonviolent campaigns become less likely under conditions of political instability. Otherwise, we would expect both no-party and single-party regimes to display positive curves. Furthermore, the graph also suggests that multiparty autocracies can be meaningfully contrasted with other autocracies in terms of the effects of regime duration. I therefore proceed with the main test of *H3*.

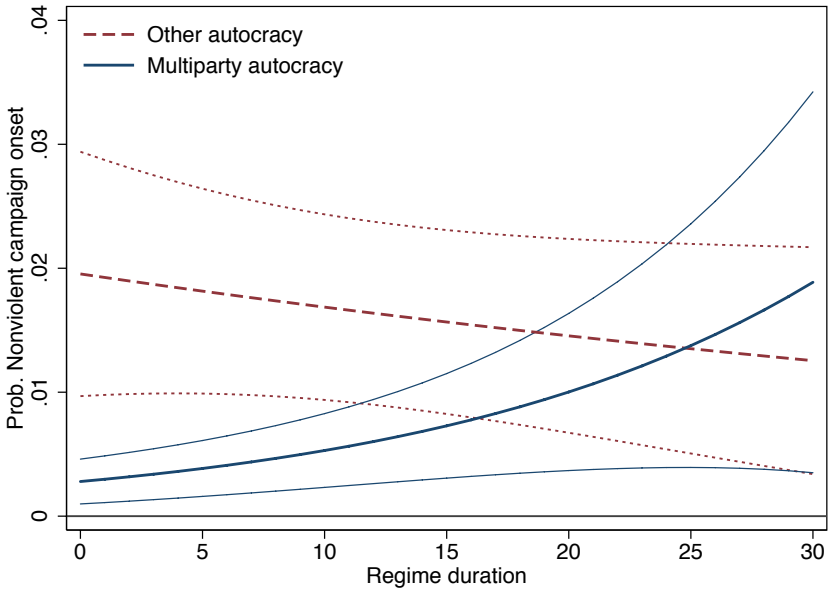
Model 8 presents the main conditional model, where I interact *Multiple parties* with *Ln regime duration*. The model offers firm support for *Hypothesis 3*. As expected, the

[57] I have restricted the graph to 30 years because of the scarcity of country-years and nonviolent campaigns in regimes that have lasted longer than this interval.

[58] Using the log-transformed variable of *Regime change* leaves somewhat greater substantial effects, but becomes harder to interpret because the variable shows relative rather than absolute increases in regime duration.

interaction term is positive and highly significant (1%-level), indicating that the risk of campaign onset in a multiparty autocracy, as compared to other autocracies, is highly conditioned on a regime's duration. However, while the coefficient estimates provide evidence supporting my expectations, it is problematic to infer directly from these estimates whether the conditional effects will hold across a meaningful range of values. Following Brambor, Clark, and Golder (2006), I therefore plot the marginal effect of the primary independent variables over the conditional variable along with associated confidence intervals.

Figure 5.5 *Conditional effects of multiparty autocracy given regime duration*



Note: The graph shows the conditional marginal effects of restrictions on political parties with 90% confidence intervals. All other covariates are held at their mean or modal values (for categorical variables).

The figure provides strong evidence of a relative increase in the probability of onset in multiparty autocracies. The effect of having multiple parties becomes statistically insignificant after about 15 years, and reaches the level of other autocracies after 25 years. Multiparty autocracies are thus significantly less likely than other autocracies to experience nonviolent campaigns for a relatively long interlude following regime change. Furthermore, the figure also lends convincing support that multiparty autocracies experience an absolute increase in the risk of onset over time. Admittedly, this increase is associated with a considerable level of uncertainty, making it hard to determine the magnitude of this effect. In large part, this is probably due to the relatively short duration of most multiparty autocracies, which generates more uncertain estimates for long-lasting regimes.

Estimating Model 8 with region and decade dummies only leads to a stronger coefficient estimate for the interaction term (see Table B.1 in Appendix B), and the association therefore does not appear to be driven by particular regional or time trends. All in all, the evidence presented here therefore leaves substantial support for *H3*, and it appears that the effect of permitting multiple parties is highly conditioned on the duration of a regime.

Finally, the estimated effects for the control variables are as expected for a majority of the covariates. Yet, only a few are able to reach conventional levels of significance across all eight models. Among the three proxies for opposition strength, *Oil and gas value* is consistently and significantly associated with a lower risk of campaign onset throughout all of the specifications, although the effect from this variable becomes much more uncertain once we control for the MENA countries. *Ln GDP per capita* fails to reach significance in any of the models, and shows little sign of any effect. A possible explanation is that economic development has little effect in the absence of urbanization (Dahl et al. 2014). Consistent with previous studies, there appears to be a positive effect of the level of urbanization on the likelihood of a popular uprising (e.g. Dahl et al. 2014; Knutsen 2014). The term for *Urban population* fails to reach significance in some of the models, but this is most likely due to the high level of multicollinearity with GDP per capita.[59] The direction and significance of the other control variables are of less interest here, and I therefore do not offer any substantive interpretation of these results. The next section will instead proceed with evaluating how well the main models fit the data and the robustness of the results.

5.3 Model Diagnostics and Robustness

5.3.1 Measures of Fit

One fundamental test of the explanatory power of any independent variables is how much they contribute in terms of prediction. As Ward, Greenhill, and Bakke (2010) have demonstrated in the case of civil wars, the level of statistical significance may be a poor indicator of how much a variable will add in terms of predictive power. Although the main concern of this study has been investigative rather than predictive, the empirical fit of the models nevertheless constitutes an important means of evaluating the validity of the results. If the models do poorly in terms of explaining variations in the data, then it indicates that the models have limited explanatory power. Here, I rely on three heuristics to assess the in-

[59] In fact, leaving out *GDP* per capita gives significant results (5%-level) in all models for *Urban population*.

sample predictive power of the main models: Aikake's Information Criterion (AIC), Receiver Operating Characteristics (ROC) curves, and separation plots. Furthermore, three models are selected for detailed comparison: Model 2 (the baseline model), Model 3 (the full model), and Model 8 (the conditional model). These three models are also compared with one model containing only the lagged dependent variable ("Previous campaign only"), as well as a model that includes only the control variables from Model 3 ("Controls only").[60]

The AIC is a commonly used to evaluate the overall efficiency of a model for a given set of data. It is similar to the Log-Likelihood, except that it punishes the inclusion of additional variables. Thus, it addresses the trade-off between the goodness of fit of a model and the complexity of the model. Crucially, it also permits comparison across models based on different samples. All else being equal, the model with the smallest AIC is considered the better fitting model (Long 1997: 109-110). Comparing the AICs in Table 5.3 and Table 5.5 reveals that the baseline model performs much better once it excludes territorial campaigns. Furthermore, the extensive model (Model 3) contributes to a substantial increase in model fit compared to the baseline models. With an AIC value of 752 this model also offers a much smaller AIC than the *Campaigns only* model (AIC=896), and a substantially lower AIC than the *Controls only* model (AIC=758). Finally, judging by AICs and Log-likelihood values, we also see that Model 8 provides a considerably better fit than Model 3. This improvement is in large part due to the inclusion of the interaction term between regime duration and multiparty autocracies.

Comparing the ROC curves for the selected models also indicate that the explanatory variables make positive, albeit moderate contributions to in-sample prediction. ROC plots visualize the relationship between the rate of false positives (defined as the number of incorrectly predicted onsets divided by the total number of cases where nonviolent campaigns did not occur) and the rate of true positives (defined as the number of correctly predicted onsets divided by the total number of cases where nonviolent campaigns did occur) at different thresholds (Ward, Greenhill, and Bakke 2010).[61] Models with more predictive power will tend to produce a higher rate of true positives (on the *y*-axis) than false positives (on the *x*-axis), generating curves that appear to be pulled toward the upper-left corner of the plot. The size of the area under the ROC curve (the "AUC score") can then be used as an

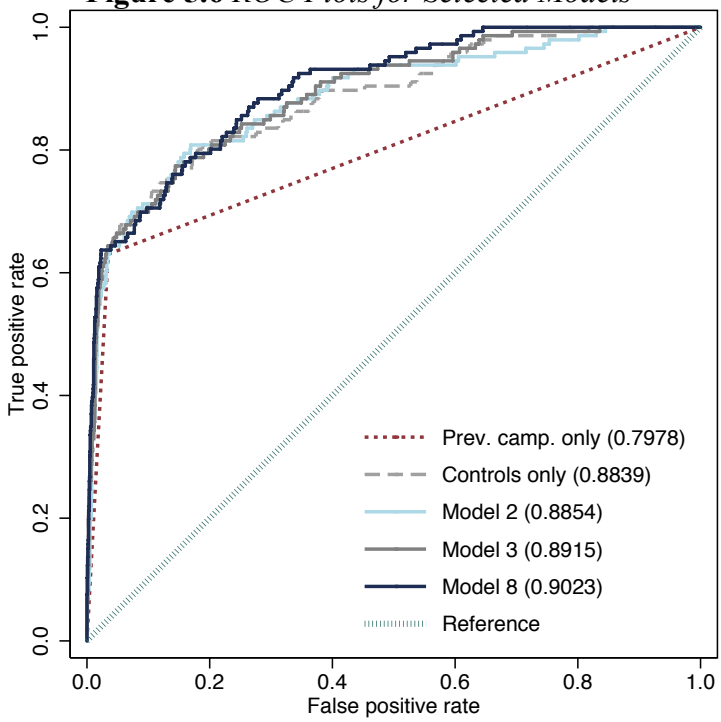
[60] Both of these models exclude territorial campaigns to permit comparison.

[61] In contrast to contingency tables, which have to assign an arbitrary predicted probability for which an event is deemed to occur or not, the ROC curve evaluates the relationship between false positives and true negatives across all possible thresholds (from 0 to 1).

indicator of the model's predictive power.[62] A perfect model that perfectly predicts all true positives at the expense of no false positives receives an AUC of 1.0, while a model that gets just as many cases wrong as right for each possible threshold receives an AUC of 0.5 (Greenhill, Ward, and Sacks 2011).

Figure 5.5 plots the ROC curves for the three selected models and the two comparison models. Again, we see that the selected models perform far better than the model with only a lagged dependent variable. Moreover, both Model 3 and Model 8 are associated with an increase in predictive power compared to Model 2 and the "Controls only" model. Admittedly, the increase is quite marginal, which normally signals that the increase in predictive power is accompanied by added complexity. However, here we also have to bear in mind that the returns in predictive power are going to diminish as we approach an AUC score of 1.00. Overall, the increases in AUC scores therefore indicate that the main models fare quite well in terms of in-sample performance. Crucially, comparing Model 3 and Model 8 to the "Controls only" model also suggests that the explanatory variables make fair contributions to the model's predictive power. Judging by the AUC scores, Model 8 seems to provide the best fit.[63]

Figure 5.6 *ROC Plots for Selected Models*



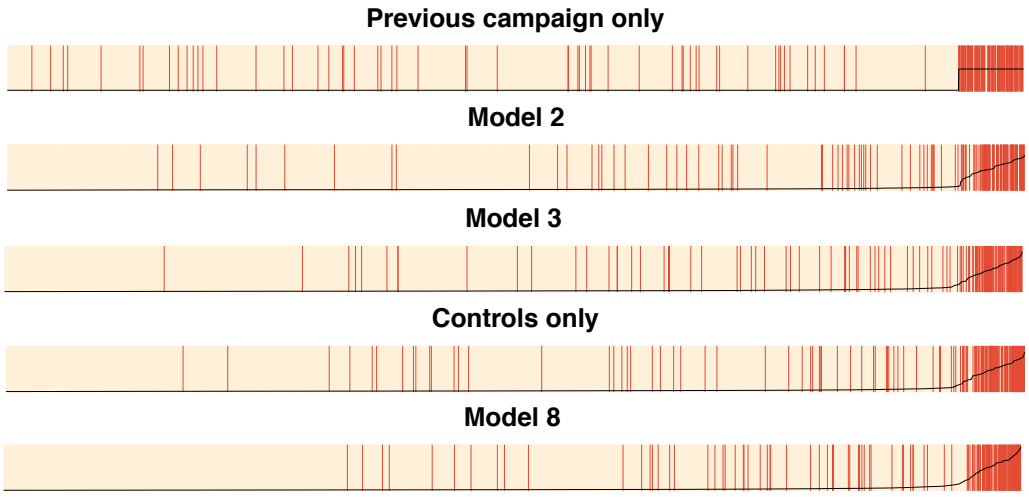
Note: AUC scores in parentheses.

As a final indicator of the models' predictive power, I also present separation plots for the respective models in Figure 5.7. The separation plot is a quite recent innovation for evaluating the fit of regression models with binary outcomes (Greenhill, Ward, and Sacks 2011). It provides a much more intuitive indicator of the performance of a model than the ROC plot or AIC scores. Separation plots are created by arranging all country-years

[62] In particular, the AUC score denotes the probability that a randomly selected positive case will have been assigned a higher probability than a randomly selected negative case (Chenoweth and Ulfelder 2015: 17).
[63] Again, this is to a large extent due to the inclusion of the interaction term. For instance, Model 6 receives an AUC score of 0.897, which only makes up half of the increase in AUC score from Model 3 to Model 8.

according to their predicted probability of nonviolent campaign onset, from lowest on the left to highest on the right. The black line that runs through the center of the graph represents this probability. Each red line corresponds to an observed positive outcome (nonviolent campaign onset), and light yellow lines represent negative outcomes (no onset). Red panels on the left are false negatives, while yellow panels on the right are false positives. Good models are thus the ones that are able to place most of the red panels at the right end of the separation plot.[64]

Figure 5.7 *Separation plots of selected models*



The separation plots in Figure 5.7 indicate that the selected model do quite well in assigning higher probabilities to events and lower probabilities to non-events, particularly compared to the model with only a lagged dependent variable.[65] The plots also show that the extensive model fits the data better than the baseline model. Moreover, although less noticeable, a larger fraction of the observed outcomes are clustered to the right in Model 3 than in the "Controls only" model, which indicates that including the party restrictions dummies is associated with a gain in predictive ability. Model 3 seems to do particularly well in terms of avoiding false positives towards the edge of the graph, i.e. among those observations that have been assigned a high probability. Again, Model 8 appears to be superior in terms of model fit. The model does remarkably well in separating the red panels from the yellow ones.

In sum, we see that the selected models seem to describe the data rather well. Nevertheless, some potential pitfalls remain. One danger is overfitting, in which case the data

[64] The separation plots were estimated in R.

[65] Note that the peculiar behavior of the "Previous campaign only" model in Figure 5.6 and Figure 5.7 is a product of predictions only being based on whether there was a campaign in the previous 3 years or not. This generates only two predicted probabilities: one for the group that experiences a campaign in the previous 3 years, and another prediction for the group that did not. This results in a straight line in both plots.

will do poorly in out-of-sample predictions. Moreover, it is questionable whether some of the observed patterns bear any relevance today. For instance, according to Wahman et al.'s (2013) data there were only 8 single-party regimes left in 2010. As such, much of this thesis describes what arguably is a historical phenomenon rather than a contemporary one. The conditional model seems to hold greater promise in this regard, given that it deals more specifically with dynamics within multiparty autocracies and no-party regimes. In particular, it would be useful to consider whether this model carries any relevance for some of the more recent uprisings, such as the nonviolent campaigns that occurred during the Arab Spring. In lack of data, I leave this task to future research.

Additional concerns

Aside from the model's predictive power, we also have to consider two remaining issues that relate to the fit of the model. The first concern is multicollinearity. High correlations among some of the covariates may produce imprecise standard errors and lead me to accept false null hypotheses (type-II error). Judging by the *Variance Inflation Factor* (VIF) score of the respective variables in Model 3 and Model 8, multicollinearity does not seem to be a troubling issue (see Table C.1 in Appendix C). VIF scores measure how much the variance of an estimated regression coefficient is increased due to collinearity. As a rule of thumb, multicollinearity is considered a problem if VIF scores exceed 10 (Kennedy 2003: 213). In my case, all VIF values are below 5, except in Model 8 where the inclusion of the interaction term lead *Multiple parties* and *Multiple parties*regime duration* to obtain VIF scores of 7.11 and 5.88. Hence, I consider this a minor threat to the validity of my results.

A second concern relates to whether my results are driven by outliers and influential observations. Outliers are observations that have unexpected values on the dependent variable given their predicted values from the estimated regression models. Influential observations are observations that have particularly large effects on the regression results. Outliers and influential observations can unduly influence the parameter estimates, a problem that is particularly severe in logistic regression models (Pregibon 1981).

To spot outliers, I look at the standardized residuals. All units that have standardized residuals of ±2 are considered outliers. Both Model 3 and Model 8 have 6 outliers: one case in each model that was assigned a high probability of onset but experienced no event, and

five cases that experienced onsets despite being assigned low probabilities of an event.[66] It is hard to detect any particular pattern among these cases, although one notable similarity is that a majority of these cases were unexpected onsets in multiparty autocracies (three observations in model 3 and four observations in model 8). Excluding these cases produces much stronger and more significant coefficient estimates for both multiparty and no-party autocracies when single-party regimes is left as the reference category (see Table C.2 in Appendix C). Thus, my findings seem robust to outliers.

In order to assess the models' vulnerability to influential units, I rely on Pregibon's (1981) influence statistic *dbeta*. This statistic is a measure of the change in coefficient estimates that would be incurred by deleting an observation. A *dbeta* greater than one is a sign that an observation has a large effect on the estimated coefficients (Menard 2009: 135-136). In my case, the observation with the highest *dbeta* received a score of 0.72 (Argentina 1977), and there are very few observations with *dbeta* scores greater than 0.4 (see Figure C.1 in Appendix C). As such, influential units do not appear to be driving my results.

5.3.2 Robustness checks

In order to ascertain that my findings are not merely a product of model specifications, the method used or the data, I run a series of robustness tests to evaluate the sensitivity of the results. Here, I will concentrate in particular on missing data, operationalizations of the dependent and independent variables, and omitted variable bias. These robustness checks are conducted on the two main models, Model 3 and Model8, and reported in Appendix D.

I start by conducting some basic sensitivity analyses. First, I test the implications of including *Democracy* as a control variable. Both models remain robust and largely unaltered (see column 1 in Tables D.1 and D.2). Next, I analyze whether the regression results are sensitive to omitting one and one control variable from the analysis (not reported here). The results are robust, with two exceptions: when *Ln oil and gas value* is left out of the analysis the coefficient for *Multiple parties* just falls below significance in Model 3 (p-value: 0.12). The coefficient for *No parties* stays almost unchanged. This appears to be due to the higher proportion of single-party regimes than multiparty autocracies with substantial natural

[66] Outliers in Model 3 with standardized residuals in parentheses: Chile 1984 (-2.01), Guyana 1990 (15.65), Senegal 2000 (10.62), Madagascar 2001 (10.82), Kyrgyzstan 2005 (10.72), and Nepal 2006 (11.52). Outliers in Model 8: South Korea 1988 (-2.21), Iran 1977 (11.19), The Philippines 1983 (9.86), Zambia 2001 (10.39), Ukraine 2001 (10.48), and Nepal 2006 (10.76).

resource revenues.[67] This is largely in line with the theoretical argument presented in Chapter 3, which stipulates that autocracies will depend on lower levels of institutionalization when they are less dependent on the opposition. Thus, the lower vulnerability to nonviolent campaigns among this subgroup of single-party regimes is arguably not attributable to their institutional framework, but instead other covariates associated with these regimes. Furthermore, when *Previous campaign* is omitted from the analysis both the coefficients for *Multiple parties* and *No parties* lose significance in Model 3. This is as expected in the case of multiparty autocracies. As discussed in Chapter 4, many autocracies have made transitions to multiparty regimes as a direct consequence of nonviolent campaigns. Thus, excluding *Previous campaign* could potentially introduce a significant endogeneity bias, as some multiparty regimes would be the outcome rather than a cause of these campaigns. However, why *No parties* loses significance is not quite as evident. In the cases of Mali (1990-1992), Niger (1991-1992) and Myanmar (1988-1990), popular mobilization – at least initially - led to authoritarian reversals and the banning of opposition parties. Thus, these instances were quite clearly cases of reverse causation. Yet, this explanation only accounts for a small fraction of the campaign onsets in no-party regimes. A second explanation may be that no-party autocracies have experienced more lengthy campaigns than single-party autocracies. While the average duration of nonviolent campaigns in single-party regimes is 2,38 years, the average campaign duration in no-party regimes is 2,60 years. Counting every campaign-year as a new onset would therefore make no-party regimes appear more vulnerable to nonviolent campaigns.[68] Given that these cases are not really new campaign *onsets*, I consider this lack of robustness as a minor threat to the validity of my results.

I investigate the impact of missingness by replacing the missing units with imputed data. As discussed in Chapter 4, the high level of missing data among single-party regimes may significantly bias my results. Running Model 3 and Model 8 with the imputed data from Amelia II confirm this assertion (see column 2 in Tables D.1 and D.2) (Honaker and King 2010). In Model 3, the difference between multiparty autocracies and single-party regimes becomes considerably larger and more significant (p-value of 0.002) when I replace the missing values with estimated values. On the other hand, the difference between single-party regimes and no-party regimes is slightly reduced but still very significant (5%-level). By

[67] Among non-communist one-party regimes the mean value on *Ln oil and gas value* is 0.212 (0.160 for all one-party regimes). In comparison, the mean value for multiparty autocracies on this variable is 0.131. Studying density estimates also reveals the same picture (not shown here).

[68] To some extent this also explains why omitting *Previous campaign* makes *Multiple parties* lose significance, given that the average campaign duration in these regimes is 2,54 years for governmental campaigns.

these estimates, single-party regimes are 3,3 times more likely to experience nonviolent campaign onsets than multiparty autocracies and 2,5 times more likely to experience an onset than no-party regimes.[69] Model 8 is also strongly supported with the imputed data, although the coefficient for the interaction term is slightly smaller than in the original sample. Hence, the findings remain robust when I replace the missing values. If anything the imputed data appear to give my hypotheses increased support.

I now proceed to examine alternative specifications of the dependent variable and independent variables. First, I investigate the effect of employing stricter inclusion criteria for nonviolent campaigns, increasing the threshold from 1,000 to 10,000 participants. The results are highly robust to this specification (see column 3 in Tables D.1 and D.2). In fact, they indicate that single-party regimes are even more prone to campaigns of this size relative to multiparty and no-party autocracies. For instance, the predicted probability of such a large-scale onset in single-party regimes is 3.2 times higher than in multiparty autocracies, and 3.8 times higher than in no-party autocracies.

Second, employing Hadenius and Teorell's (2007) original typology also offers an alternative classification of the explanatory variables. The strength with this classification is that it allows me to investigate whether the results hold when we focus on pure single-party and multiparty autocracies, instead of all dictatorships with one party or all dictatorships with several parties. Column 4 in Table D.1 and Table D.2 reports the results from this analysis. Again, we see that single-party regimes have a significantly higher probability of onset than multiparty autocracies. Furthermore, the positive coefficient for *Monarchy* and *Other dictatorship* corroborate the finding that no-party regimes have a lower risk of campaign onset than single-party regimes.[70]

Third, I consider the effects of replacing the regime dummies from Wahman et al. (2013) with data from Cheibub et al. (2010), who employ different coding criteria for democracies, military regimes and the three levels of institutionalization. The results remain substantially unaltered from this alternative classification (see column 5 in Tables D.1 and D.2).

Fourth, I consider two alternative ways of operationalizing *Previous campaign*: a log-transformed measure of time since last campaign and a decay function of the proximity of nonviolent conflict. The first approach is concomitant to the one applied in Hegre and

[69] I used *Clarify* to obtain these predictions.

[70] Admittedly, the *Monarchy* dummy does not reach significance. However, this appears to be a product of the low number of observations on this variable. The only nonviolent campaigns to take place in a monarchy with no parties – which constitute 85% of all monarchies - were the two campaigns in Nepal in 1990 and 2006.

Nygård's (2014) study of armed conflict recurrence, and is simply the log of the number of years without a campaign (or since independence) up to *t - 1*. The second approach is equivalent to the one taken by Hegre et al. (2001) and Fjelde (2010), and specifies an exponential function of the time that has passed without the onset of a nonviolent campaign, which is given by $2^{(-\text{time since last transition}/\alpha)}$, where α is the half-life parameter. In my case, a half-life parameter of 2 years provided the best fit.[71] By combining either of these measures with a one-year lag of the dependent variable, one can account for both ongoing campaigns and long-term path dependency (Hegre and Nygård 2014).[72] As shown in Column 6 and 7 in Tables D.1 and D.2, the significance and impact of the explanatory variables remain substantially unaltered with either of the two approaches. Moreover, the two measures of time dependence do not reveal any consistent or significant temporal effects, which suggest that there is little evidence that previous campaigns have an impact on the onset of new campaigns. This is compatible with Dahl et al.'s (2014) findings.

Fifth, it is possible that multiparty autocracies face fewer nonviolent campaigns because they experience more small-scale armed conflicts (Fjelde 2010). I therefore replace the measure on civil wars from NAVCO 1.0 with UCDP/PRIO's measure of internal armed conflicts (Themnér and Wallensteen 2014) This lowers the threshold for armed conflicts from 1,000 to 25 battle-related deaths. The results indicate that this only leads to a minor reduction of the coefficient for *Multiple parties* in Model 3. Otherwise, the main estimates remain largely unaltered for both Model 3 and Model 8 (see column 8 in Tables D.1 and D.2). As such, the lower risk of nonviolent uprisings in multiparty autocracies does not seem to reflect their higher vulnerability to armed conflict.

Sixth, the results remain robust if I substitute the binary logit models with probit models or multinomial logit models (see Tables D.3 and D.4). In the latter case, the dependent variable is extended to four categories: (0) no event, (1) nonviolent campaign, (2) violent campaign, and (3) both nonviolent campaign and violent campaign.

Finally, it is perfectly conceivable that country-specific effects not accounted for could affect both regime type and the risk of campaign onset, leading to biased results and invalid inferences. Moreover, as noted in Chapter 4, logistic regression models are particularly sensitive to omitted variable bias, regardless of whether the excluded variables are related to the explanatory variables or not (Mood 2010). A common way of addressing the problem of omitted variable bias is to run fixed effects models, controlling for all country-specific and/or

[71] This was established by comparing the log-likelihood values of Model 3 with various half-life parameters.
[72] Note that both measures were backdated to 1946.

time-specific effects. However, because the majority of countries never have experienced a nonviolent campaign between 1972 and 2006, running regressions with country fixed effects is equivalent to discarding all information from these countries when estimating effects (Beck and Katz 2001; Knutsen 2014: 927).[73] Similarly, there are some years with very few observations on the dependent variable, which complicates time fixed effects regressions (Stock and Watson 2012: 400-403). Instead, to control for unobserved heterogeneity, I estimate random effects models for Model 3 and Model 8. This estimation method allows both intercepts and slope coefficients to vary across groups of observations, capturing the effects of unobserved or omitted country-level factors. Crucially, the estimated effects remain very robust to this specification. In fact, the coefficients for *No parties* and *Multiple parties* increase substantially in size and significance in Model 3 (both significant at the 1%-level) (see column 1 in Table D.5). The same happens with the interaction term for *Multiparty*regime duration* in Model 3 (see column 1 in Table D.6). Thus, running random effects models only strengthens the credibility of my results.

Moreover, the results remain robust when I control for other factors that could affect the association between regime type and nonviolent campaign onsets, including the proportion of revolutions globally, the length of the chief executive's tenure, the level of active repression, the level of welfare spending, economic growth, the Cold War, the level of civil liberties and the level of democracy (see columns 2-4 in Tables D.5 and D.6).[74]

In sum, the findings appear to be robust to a broad range of model specifications and operationalizations. *H2* and *H3* receive consistent and strong support in all of the robustness checks, and *H1* receives solid backing through the vast majority of alternative specifications.[75] It therefore seems safe to conclude that the results are not to driven by the choice of method, variables or data.

[73] Note, however, that running a fixed effects leaves strong support for Hypotheses 1, 2 and 3, producing stronger and more significant estimates for all of the explanatory variables (not shown here).

[74] The proportion of revolutions globally captures the proportion of other countries experiencing a nonviolent campaign globally. The variable on the length of the chief executive's tenure is drawn from Cheibub et al. (2010), and has here been log-transformed. The variable on repression is drawn from the US State Department's Political Terror Scale (Gibney, Cornett, and Wood 2010). The variable on welfare spending is retrieved from (Taydas and Peksen 2012) and measures the level of expenditure on welfare policies (i.e. education, health, and social security) to GDP. The variable is imputed and log-transformed. Finally, I have also included Freedom House's measure of civil liberties, as well as a centered and a squared term of the level of democracy generated from the Scalar Index of Polities (SIP) (Gates, Hegre, Jones, and Strand 2006). The SIP index is used as an alternative to the Polity index, given that the Polity index includes factionalism in the midrange categories.

[75] Although not shown here, the difference between no-party and multiparty autocracies is insignificant through all specifications.

5.4 Assessing the Evidence: Do Institutions Channel Contention?

To summarize, the findings of this empirical analysis are consistent with the theoretical stipulations made in Chapter 3. First, dictatorships with one party run a significantly higher risk of nonviolent campaign onset than both no-party and multiparty autocracies. Second, I also find strong evidence that multiparty autocracies run no higher risk of onset than dictatorships that ban political parties. Third, we also see that the likelihood of campaign onset is highly conditioned on the duration of a regime. Dictatorships with multiple parties run a significantly higher risk of onset as time progresses.

The effects are also relatively robust and comparatively strong. Thus, there appears to be considerable evidence that institutions matter in channeling contention. Despite featuring conditions that should make it easier to mobilize popular resistance, multiparty autocracies still experience a comparatively low exposure to nonviolent uprisings. In comparison, single-party regimes have experienced a relatively high frequency of nonviolent campaign onsets, which appears to support the notion that these regimes will tend to underinstitutionalize to maintain internal stability.

The odd case here is dictatorships with no parties. As hypothesized, these autocracies seem to be much less vulnerable to nonviolent uprisings than single-party regimes. This finding goes to show that the relationships covered do not simply reflect an association between the level of democracy and the risk of nonviolent conflict. No-party regimes tend to be staunchly authoritarian, yet feature no higher risk of uprisings than dictatorships with multiple parties. Arguably, this demonstrates the importance of opposition strength for mobilizing nonviolent action.

Alternatively, no-party regimes may experience a lower risk of campaign onset because they are more capable of suppressing dissent. Following this logic, no-party regimes ban opposition parties not because they face a weak opposition, but because they have strong confidence in their ability to repress mobilization before it escalates. However, I have controlled for several of the most common proxies of regime capacity, including GDP per capita, military personnel and the level of democracy. Most likely, the effects of opposition strength and regime capacity are likely to work in conjunction. As discussed in Chapter 3, these two factors are often two sides of the same coin: if the regime is strong, we also tend to find that the opposition has limited leverage and mobilization strength.

Although the finding that no-party regimes experience a relatively low likelihood of uprisings may be discouraging from an activist's point of view, these regimes are far from invulnerable to popular uprisings. In fact, no-party autocracies appear to be particularly susceptible to revolutionary contagion. The only interaction effect between the explanatory variables and the control variables that appeared positive was one between neighboring campaigns and no-party regimes (see Table B.2 in Appendix B). The results also show that dictatorships with no parties experience a much higher risk of nonviolent uprisings than democracies.

Finally, the fact that multiparty autocracies become more exposed to nonviolent campaigns over time supports the notion that it is first of all the presence of opposition parties and multiparty elections that precludes popular mobilization in multiparty autocracies. A competing explanation is that multiparty autocracies dampen dissent by providing more public goods (e.g. Bueno de Mesquita and Smith 2010). However, the findings here are highly significant also when I control for the level of welfare spending to GDP. Moreover, economic productivity tends to increase over time, and hence also welfare provisions (Bueno de Mesquita and Smith 2010; Gandhi 2008). Thus, the previous line of reasoning would lead us to expect multiparty autocracies to face a lower risk of onset over time, not a higher risk. Admittedly, regime duration could be indicative of more personalized regimes, which are associated with lower levels of economic growth and public service provision (Wright 2008). Yet, I still find a very strong interaction effect even when controlling for executive tenure. Thus, it does appear that permitting opposition parties does play a role in channeling contention, even if this effect is highly contingent on the duration of the regime. Conversely, failing to respond with the appropriate level of institutionalization could significantly heighten the risk of a nonviolent uprising.

5.5 Summary

This chapter has empirically tested the effects of restrictions on political parties on nonviolent uprisings. Consistent with the expectations from *Hypothesis 1*, the analysis showed that single-party regimes are more likely than both no-party and multiparty autocracies to experience nonviolent campaigns over regime change. Furthermore, in the absence of external shocks, dictatorships with no parties and dictatorships with multiple parties experience the same risk of governmental campaign onsets, thus confirming *Hypothesis 2*.

Finally, we also see that the effect of restrictions on political parties is highly dependent on the time since regime change. As stipulated in *Hypothesis 3*, multiparty autocracies experience a substantial increase in the risk of onset over time compared to other autocracies. In general, I therefore find strong support for all three hypotheses, even if their relevance is restricted to governmental campaigns. Hence, the findings suggest that differentiating between dictatorships with no parties, one party and several parties makes a valuable contribution to mapping the onset of nonviolent campaigns.

6 Conclusion

> *None of us know all the potentialities that slumber in the spirit of the population, or all the ways in which that population can surprise us when there is the right interplay of events.*
>
> Václav Havel (1990: 109)

In this thesis, I have investigated one hitherto unexplored question in quantitative research on civil resistance: what are the effects of political party restrictions on the initiation of nonviolent campaigns in authoritarian regimes? Previous studies have largely ignored the considerable institutional differences that exist between dictatorships, thereby juxtaposing the risk of nonviolent uprisings in these regimes. Building on previous work on authoritarian survival, I have advanced three major arguments.

First, I have argued that single-party regimes should be particularly prone to nonviolent uprisings because the internal power-sharing dynamics of these regimes make them liable to 'underinstitutionalize' in the face of stronger popular opposition. If the dictator responds with greater levels of institutionalization, he also risks being overthrown by members of his ruling coalition.

Second, I have suggested that the presence of multiparty competition makes key segments of the opposition more inclined to pursue change through institutional channels over nonviolent direct action. Consequently, multiparty autocracies should face no higher likelihood of nonviolent uprisings than no-party regimes, despite the fact that no-party regimes tend to face weaker opposition movements.

Third, I have argued that the ability of multiparty autocracies to co-opt the opposition is highly conditioned on the proximity to the last regime change, and that the stabilizing properties of allowing multiple parties should decline over time. Hence, the effects of regime duration should be higher in multiparty autocracies than in other autocracies, which generally tend to experience more instability at the initial stages of their tenure.

The evidence presented here provides significant support for all three arguments. First, single-party regimes experience a significantly higher risk of nonviolent uprisings than both multiparty autocracies and no-party regimes and this difference appears to be quite substantial. Second, multiparty autocracies and no-party regimes experience practically the same likelihood of nonviolent uprisings throughout all model specifications, and the

differences between the two regime types are never statistically significant. Third, the effect of regime duration on the likelihood of a nonviolent campaign onset was found to be much higher in multiparty autocracies than in autocracies without opposition parties. Although their relevance appears to be restricted to governmental campaigns, these associations appear to be robust and provide fair contributions to in-sample performance. As such, I conclude that there is credible evidence of a 'channeling effect': political party restrictions do appear to matter for whether, when and why opposition movements pursue regime change through nonviolent direct action. The high levels of civil resistance in authoritarian regimes can thus at least in part be attributed to their inability to channel contention, as compared to democracies – an effect that is exacerbated by the fact that autocracies generally tend to generate more disputes. In the remaining sections I highlight some of the main implications and limitations of the main arguments, and suggest some potential directions for future research.

6.1 Implications

The arguments presented here have several implications both for academic research on the subject and for policy. First, my results suggest that scholars should exercise more caution when discussing the stability of different regime types. A recurring tendency in the literature on authoritarian survival has been to highlight the resilience of single-party regimes and the volatility of multiparty autocracies on the basis of aggregate survival rates (Hadenius and Teorell 2007; Magaloni 2008). However, the fact that single-party regimes experience a higher likelihood of nonviolent uprisings than multiparty autocracies demonstrates the importance of specifying the sources of instability. Because dictators rely on different co-optation and repression strategies, they are also likely to struggle with different threats to their rule. Importantly, more recent studies have acknowledged this possibility, and have started to compare different sources of autocratic breakdown (e.g. Geddes et al. 2014; Goemans et al. 2009; Svolik 2012). Certainly, this is a fruitful area for future research.

Another set of implications relates to the role of temporal dynamics. How long a regime has lasted seems to play significant roles in conditioning the effects of political institutions on nonviolent uprisings. Previous accounts of the effects of political party restrictions are often highly static, and offer little consideration of how these effects vary over time (Magaloni 2008; Svolik 2012). This implies that the effects of regime duration may be far more complex than what has previously been assumed (see also Chenoweth and Ulfelder 2015).

The finding that multiparty autocracies experience a higher risk of nonviolent uprisings over time also leads to an important policy implication: Unless dictators find new and innovative ways to counter popular opposition, I do not expect the current wave of nonviolent uprisings to subside anytime soon. Given that multiparty autocracies now constitute about 75% of all dictatorships (Wahman et al. 2013), the dynamics in these regimes should carry significant relevance for future trends in nonviolent uprisings. Moreover, a relatively large fraction of these regimes have only quite recently made transitions from other autocracies.[76] Hence, in the absence of democratic progress, it is quite conceivable that opposition movements in these countries will mobilize nonviolent direct action. Commenting on the upsurge in popular uprisings witnessed in the last few decades, Kendall–Taylor and Frantz (2014: 35) ask whether these "revolts [are] evidence that autocrats are becoming increasingly vulnerable to the masses? Or are they short-term exceptions to a longer-standing rule of autocratic ouster?" Although the findings here obviously cannot offer any conclusive evidence, they support the notion that these episodes are indicative of a more long-term trend.

Finally, my study also suggests that opposition movements in no-party regimes may be particularly dependent on outside assistance to facilitate the mobilization of nonviolent campaigns. These movements can benefit substantially from aid such as material assistance to recruitment purposes, technical capacity building and the distribution of information - all factors that can help strengthen the membership base of these movements (Chenoweth and Stephan 2011). Without numbers the movements are unlikely to accomplish any major policy change. With numbers, however, they can constitute significant forces, regardless of whether the regime needs their support or not. Moreover, the risk of violent repression is likely to decrease as these movements extend their membership base. As the head of the East German security apparatus allegedly said to the party leader, Erich Honecker, during the uprisings of 1989: "Erich, we can't beat up hundreds of thousands of people" (Przeworski 1991: 64).

6.2 Limitations

Some notable limitations with my study also warrant attention. As noted in Chapter 5, the NAVCO 2.0 dataset only provides coverage until 2006. As such, the recent wave of nonviolent uprisings during the Arab Spring is not included in this analysis. The factors that

[76] For instance, in 2010 about 33% of all multiparty autocracies had lasted less than 7 years if we exclude transitions from democratic regimes (Wahman et al. 2013).

precipitated these campaigns may have been quite different from those that led to most of the campaigns included in NAVCO 2.0, a subject that has been hotly debated in recent years (e.g. Bellin 2012; Chenoweth and Stephan 2014; Goldstone 2011; Lynch 2013). This is a potent issue, which can only be properly assessed through more detailed analysis.

Overall, it is unclear whether all of the findings have relevance for recent developments. This point becomes even more salient in the light of the current composition of most autocracies, as only ¼ of dictatorships retain *de facto* bans on opposition parties. These regimes may be more robust than many of the single-party and no-party regimes in the period under study. For instance, the majority of single-party regimes that still exist are communist regimes (Cheibub et al. 2010), which appear to be less vulnerable to popular pressures than noncommunist single-party regimes. Similarly, the majority of today's no-party regimes are monarchies, which have been highlighted as particularly resilient to domestic demands (e.g. Hadenius and Teorell 2007; Herb 1999). On the other hand, the predictions related to multiparty autocracies appear to carry significant relevance given the increasing prevalence of multiple parties in authoritarian regimes. Again, the only way to address these questions is by studying new data. Hence, I leave this task to future research.

Another limitation with this study is that if focuses solely on contextual variables. Several of these variables, such as level of economic development or the level of military involvement in politics, can often be highly static and not subject to much change. I have tried to address this problem by considering the interaction between regime duration and political party restrictions. This allows for a more dynamic model. Nevertheless, there are palpable reasons to expect agency to play a central role in the onset of nonviolent campaigns (see Chenoweth and Ulfelder 2015 for a more detailed discussion). As such, I contend that the patterns studied here do not predetermine the site or timing of nonviolent uprisings. However, they reveal some strong tendencies that merit more attention.

Finally, as discussed in Chapter 4, endogeneity constitutes the greatest threat to the validity of my results. This threat is practically inescapable in studies of political institutions (Hadenius and Teorell 2005; Pepinsky 2014), but this does not make it less of a pressing issue. Identifying an appropriate instrument that is both highly correlated to the explanatory variables and exogenous to the dependent variable would leave even greater confidence in my results if they remain robust (Stock and Watson 2012). However, tracking such as instrument is cumbersome, and despite my best efforts at identifying one, it has simply not been achievable within the scope of this study. Still, there are tenable reasons to expect the results to hold also in an instrumental variable approach. As previous studies have shown,

dictatorships tend to rely on a higher level of party institutionalization in conditions where the opposition is strong and the regime is more dependent on cooperation (Gandhi 2008; Gandhi and Przeworski 2006; Wright 2008). Thus, if the results were solely a product of endogeneity we would expect regimes with greater levels of institutionalization to be associated with a greater risk of nonviolent uprisings. Yet, the findings here reveal no such association. Thus, I argue that there is credible evidence that the influence of political institutions on nonviolent uprisings is not merely epiphenomenal.

6.3 Future Research

Overall, the onset of nonviolent campaigns still remains uncharted territory. Several dynamics are worth further exploration. One area that appears to be especially ripe for more research is the association between elections and nonviolent uprisings in multiparty autocracies. Electoral revolutions have received some attention in the literature on civil resistance (e.g. Bunce and Wolchik 2006; Nepstad 2011). Yet, quantitative work on these events has been practically absent.[77] As argued in this thesis, elections in authoritarian regimes are often more than empty displays, and can lower the incentives of opposition movements for nonviolent action. Still, if elections are revealed as outright shams and opposition movements are unable to make progress through these institutions, then the opposition may be inclined to pursue change through other means. Thus, elections can both provoke and preclude nonviolent action.

A second area that merits further exploration is the dynamics of no-party regimes. Previous studies of political party restrictions have tended to highlight the properties of party regimes, and have offered less attention to the inner and outer workings of dictatorships that ban opposition parties (e.g. Hadenius and Teorell 2007; Svolik 2012). The go-to reference for this question has typically been the works by Gandhi and Przeworski (2006) and Gandhi (2008). Yet, compared to all of the work that has been done on political party regimes, the dynamics of no-party regimes has received limited attention.

Finally, the prospects of democratic consolidation following nonviolent campaigns has started to receive more attention (e.g. Geddes et al. 2014), but remains relatively underexplored compared to the immediate success of nonviolent campaigns (Chenoweth and

[77] Partial exceptions include Butcher and Svensson (2014) and Chenoweth and Ulfelder (2015). Yet, none of these studies have elections as their primary interest, and offer only brief considerations of its effects.

Stephan 2011; Shaykhutdinov 2010; Svensson and Lindgren 2011).[78] Furthermore, how a regime's institutional heritage plays into this calculation has not received any attention. For instance, it is conceivable that nonviolent campaigns not only improve the prospects of democratic consolidation in general, but that these events also ease consolidation in countries with a difficult institutional heritage.

[78] Note, however, that the subject is briefly addressed in Chenoweth and Stephan (2011).

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Appendix A

Additional statistics

Figure A.1 *Histograms of Regime duration, autocracies only*

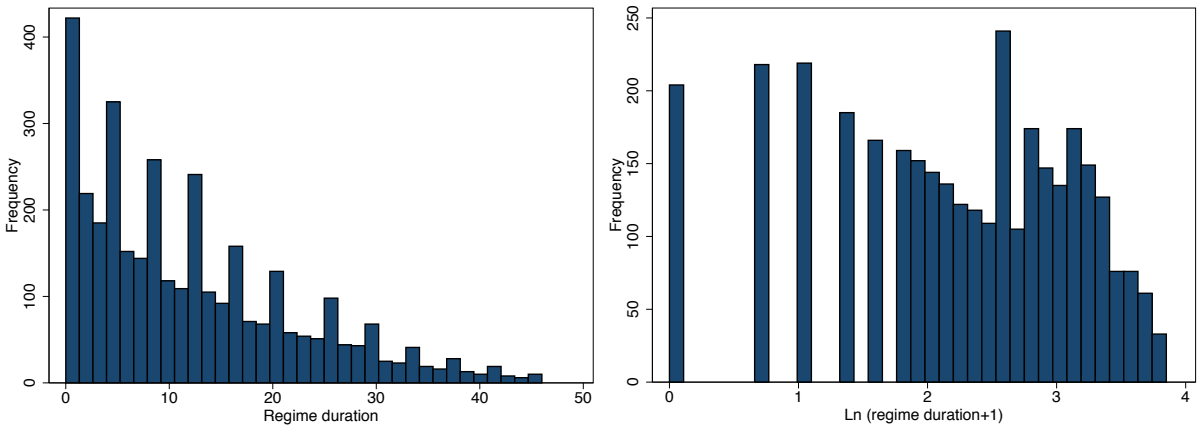


Figure A.2 *Joint distribution of restrictions on political parties and military involvement in authoritarian politics*

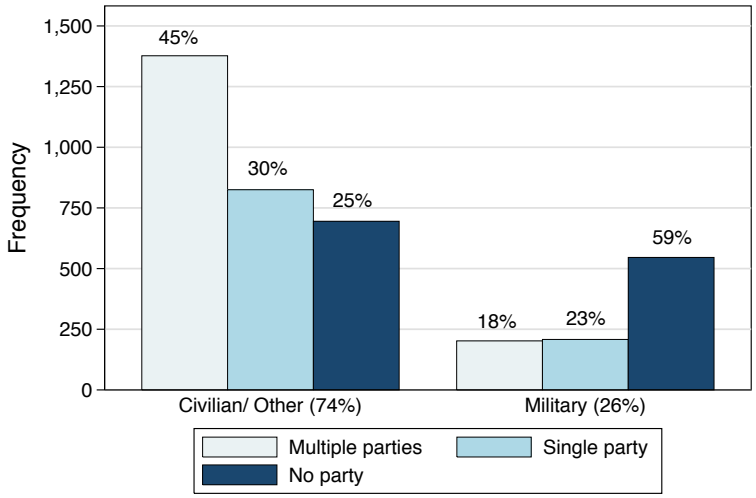


Table A.1 Descriptive statistics for continuous variables 1973-2006, all regimes

Variable	Mean	Std. Dev.	Min.	Max.	N	Missing
Ln Regime duration$_{t-1}$	2.212	0.999	0	3.850	5752	0
Ln GDP per capita$_{t-1}$	8.341	1.212	4.889	13.237	5651	101
Urban population$_{t-1}$	48.229	24.151	2.716	100	5531	221
Ln Natural resources$_{t-1}$	0.251	0.615	0	4.502	5191	561
Ln Population	15.471	1.987	9.756	21.006	5675	77
Democratic neighborhood$_{t-1}$	0.383	0.330	0	1	5706	46
Ethnic fractionalization	0.441	0.264	0	0.930	5539	213
Religious fractionalization	0.434	0.240	0.002	0.860	5578	174
Ln Military personnel$_{t-1}$	3.357	1.920	0	8.466	5598	154

Table A.2 *Frequency tables for binary variables 1973-2006, all regimes*

Variable	0	1	Total	Missing
Nonviolent campaign onset	5516	236	5752	0
No-party regime$_{t-1}$	4597	1155	5752	0
Single-party regime$_{t-1}$	4761	991	5752	0
Multiparty autocracy$_{-1}$	4468	1284	5752	0
Democracy$_{t-1}$	3430	2322	5752	0
Military regime$_{t-1}$	4468	888	5752	0
Election-year	3745	1278	5023	224
Neighboring campaign $_{t-1}$	4127	1583	5710	42
Communist $_{t-1}$	5381	361	5742	10
Civil war$_{t-1}$	4914	838	5752	0

Table A.3 *Frequency of country-years for decades, all regimes*

Decade	Absolute frequency	Relative frequency
1973-79	1,042	0.18
1980s	1,618	0.28
1990s	1,793	0.31
2000-2006	1,299	0.23
N	5,752	

Table A.4 *Frequency of country-years for regions, all regimes*

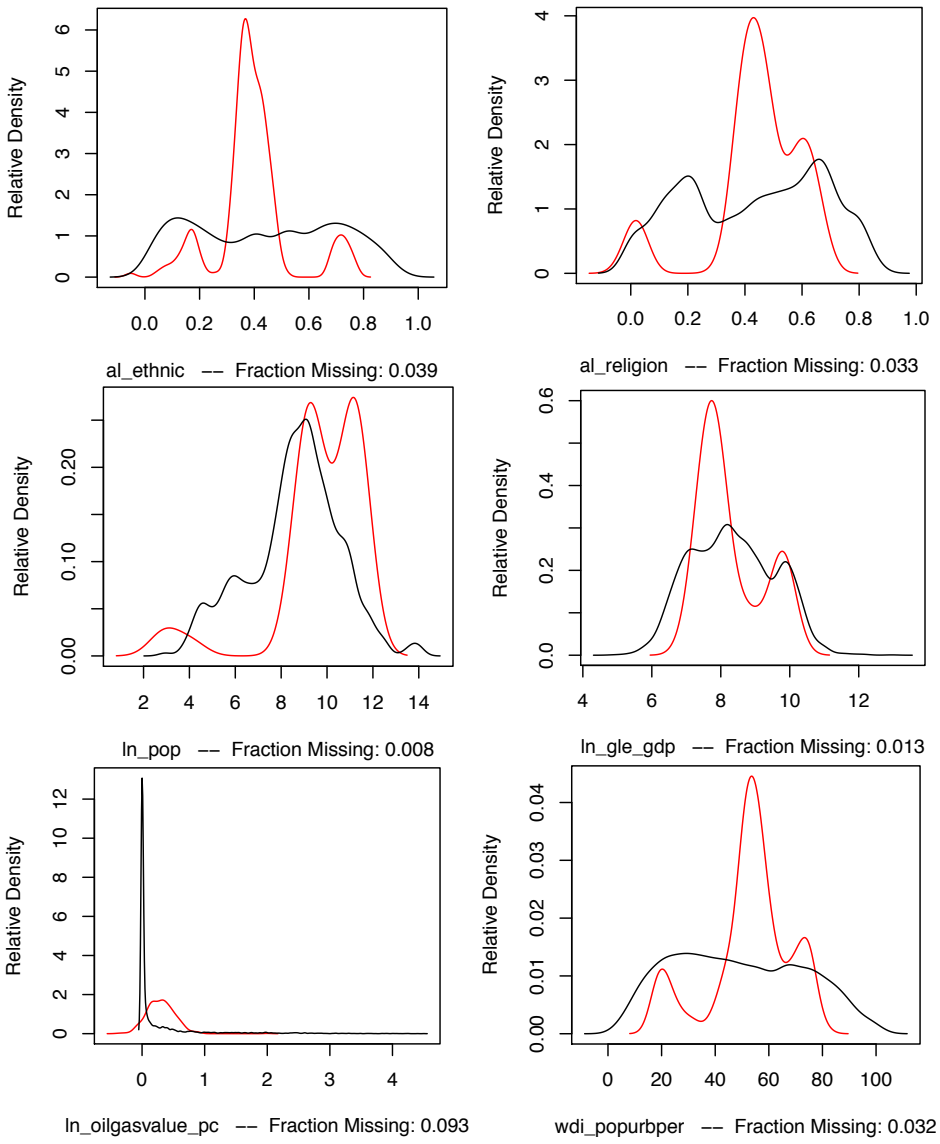
Region	Absolute frequency	Relative frequency
W. Europe & N. America (incl. AU & NZ)	782	0.14
E. Europe & ex. Soviet Union	568	0.10
Sub-Saharan Africa	1,567	0.27
Asia & the Pacific	1,067	0.19
MENA	697	0.12
Latin America & the Caribbean	1,067	0.19
N	5,752	

Table A.5 *More than 5 country-years missing, selected variables*

	Urban population	Natural resource revenue	Ethnic fractionalization	Religious fractionalization
Czechoslovakia	X		X	X
Ethiopia			X	X
Germany, East	X	X	X	X
Serbia and Montenegro	X	X	X	X
Taiwan	X	X		
USSR	X	X	X	X
Yemen, North	X	X	X	X
Yemen, South	X	X	X	X
Yugoslavia	X			

Excluding following microstates: Antigua and Barbuda, Grenada, Maldives Samoa, Sao Tome and Principe, Seychelles, and Vanuatu.

Figure A.3 *Density Plots of observed and imputed values*



Appendix B

Auxiliary regression tables

Table B.1 *Region & Decade dummies*

	Model 2	*Model 5*	*Model 8*
No parties$_{t-1}$	-0.823**	-1.229***	-1.042***
	(0.407)	(0.407)	(0.403)
Multiple parties$_{t-1}$	-0.890**	-1.146***	-3.617***
	(0.389)	(0.414)	(0.867)
Ln regime duration$_{t-1}$			0.0938
			(0.204)
*Multiparty*regime duration*			1.162***
			(0.345)
Controls			
Military regime$_{t-1}$	0.624**	0.528*	0.292
	(0.262)	(0.293)	(0.330)
Ln GDP per capita$_{t-1}$	0.475	0.212	-0.196
	(0.303)	(0.305)	(0.275)
Ln oil and gas income$_{t-1}$	-1.135*	-1.534*	-1.259
	(0.590)	(0.834)	(0.955)
Urban population$_{t-1}$	0.00198	0.0113	0.0173
	(0.0114)	(0.0112)	(0.0121)
Population$_{t-1}$	0.251**	0.464***	0.443***
	(0.113)	(0.106)	(0.115)
Democratic neighbors$_{t-1}$		-0.329	0.0748
		(0.662)	(0.677)
Neighboring campaign$_{t-1}$		0.607**	0.674**
		(0.280)	(0.270)
Ethnic fractionalization		0.176	0.270
		(0.634)	(0.686)
Religious fractionalization		-1.560***	-1.539***
		(0.554)	(0.507)
Communist$_{t-1}$		-0.882	-1.164*
		(0.661)	(0.653)
Civil war$_{t-1}$		-0.600*	-0.610*
		(0.314)	(0.353)
Previous campaign	3.560***	3.362***	3.368***
	(0.286)	(0.266)	(0.289)
1970s	-1.146**	-0.868	-0.478
	(0.582)	(0.649)	(0.662)
1980s	-0.603	-0.331	-0.0786
	(0.429)	(0.434)	(0.450)
1990s	-0.523	-0.373	-0.111
	(0.393)	(0.399)	(0.373)
W. Europe & N. America (incl. AU & NZ)	-0.211	-0.109	1.124
	(0.687)	(0.696)	(0.901)
E. Europe & ex.-Soviet	-1.201**	-0.897	-0.00351
	(0.545)	(0.567)	(0.587)
Sub-Saharan Africa	-1.137**	-1.099*	-0.994*
	(0.457)	(0.578)	(0.522)
Asia and the Pacific	-1.706***	-2.010***	-2.272***
	(0.589)	(0.545)	(0.590)
MENA	-1.363***	-1.610***	-1.272**
	(0.456)	(0.482)	(0.510)
Intercept	-7.860***	-7.444***	-5.214***
	(2.217)	(2.378)	(1.976)

Log-likelihood	-368.28	-348.24	-328.77
AIC	770.57	742.48	707.54
Observations	3,066	3,010	3,010
Countries	128	127	127

Notes: All models are logit models with *Single party* as ref. cat. For the decade and region dummies, the reference categories are the *2000s* and *Latin America & The Caribbean*. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.2 *Interaction between No parties and neighboring campaign*

No parties$_{t-1}$	-0.494
	(0.478)
Multiple parties$_{t-1}$	0.911**
	(0.389)
Controls	
Military regime$_{t-1}$	0.620**
	(0.278)
Ln GDP per capita$_{t-1}$	0.152
	(0.211)
Ln oil and gas income$_{t-1}$	-1.715***
	(0.607)
Urban population$_{t-1}$	0.0177*
	(0.00912)
Population$_{t-1}$	0.448***
	(0.102)
Democratic neighbors$_{t-1}$	1.124**
	(0.569)
Neighboring campaign$_{t-1}$	0.326
	(0.316)
*No-party*neighboring campaign*	1.046**
	(0.441)
Ethnic fractionalization	0.855
	(0.569)
Religious fractionalization	-0.985**
	(0.489)
Communist$_{t-1}$	-0.691
	(0.564)
Civil war$_{t-1}$	-0.390
	(0.283)
Previous campaign	3.400***
	(0.223)
Intercept	-10.41***
	(2.076)
Log-likelihood	-394.12
AIC	820.24
Observations	3,039
Countries	127

Notes: The estimates are based on a logistic regression model with *Single party* as the reference category. None of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix C

Model Diagnostics

Table C.1 *VIF values for variables in Model 3 and Model 8*

	VIF	
Variable	**Model 3**	**Model 8**
No parties$_{t-1}$	2.00	2.04
Multiple parties$_{t-1}$	2.21	7.11
Ln Regime duration$_{t-1}$		1.86
Multiple parties*regime duration		5.88
Military regime$_{t-1}$	1.37	1.38
Ln GDP per capita$_{t-1}$	4.15	4.26
Ln urban population$_{t-1}$	2.57	2.99
Oil and gas value$_{t-1}$	2.85	2.60
Population$_{t-1}$	1.36	1.41
Democratic neighbors$_{t-1}$	1.32	1.33
Neighboring campaign$_{t-1}$	1.05	1.05
Ethnic fractionalization	1.43	1.44
Religious fractionalization	1.13	1.13
Communist$_{t-1}$	1.59	1.59
Civil war$_{t-1}$	1.14	1.15
Previous campaign	1.09	1.09
Mean VIF	1.80	2.40

Figure C.1 *Influential units (dbeta)*

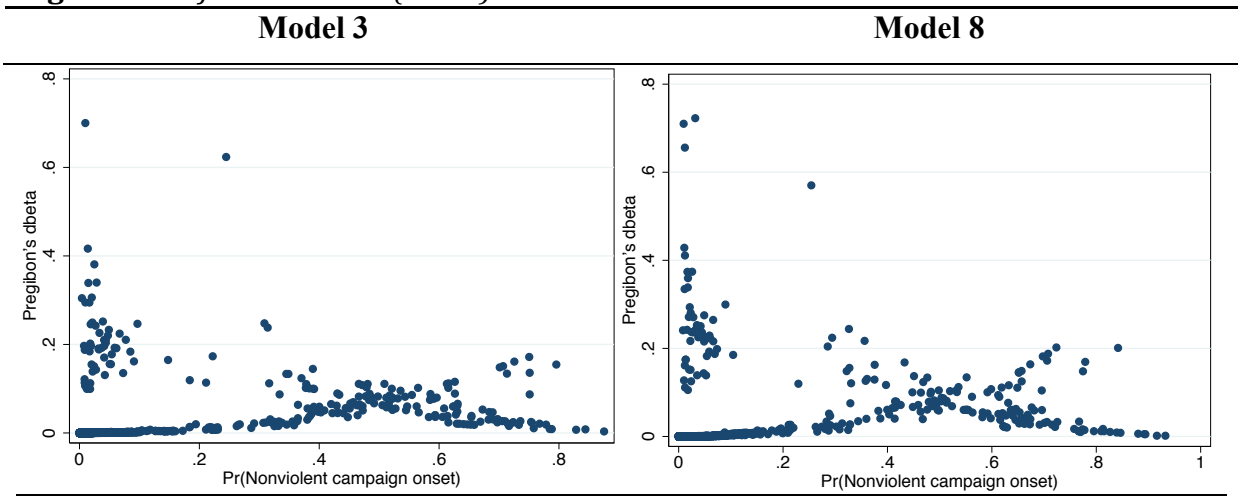


Table C.2 *Excluding outliers in Model 3 and Model 8*

	Model 3	*Model 8*
No parties$_{t-1}$	-1.121***	-1.239***
	(0.408)	(0.393)
Multiple parties$_{t-1}$	-1.245***	-3.811***
	(0.432)	(0.686)
Ln Regime duration$_{t-1}$		-0.235
		(0.195)
*Multiple parties*regime duration*		1.280***
		(0.263)
Controls		
Military regime$_{t-1}$	0.706**	0.795***
	(0.321)	(0.305)
Ln GDP per capita$_{t-1}$	0.156	-0.0223
	(0.239)	(0.253)
Ln oil and gas income$_{t-1}$	-1.906***	-1.723***
	(0.700)	(0.619)
Urban population$_{t-1}$	0.0256**	0.0212*
	(0.0101)	(0.0111)
Population$_{t-1}$	0.406***	0.317***
	(0.108)	(0.107)
Democratic neighbors$_{t-1}$	0.758	0.863
	(0.608)	(0.634)
Neighboring campaign$_{t-1}$	0.735**	0.728**
	(0.297)	(0.300)
Ethnic fractionalization	0.481	0.334
	(0.648)	(0.583)
Religious fractionalization	-1.193**	-0.836
	(0.601)	(0.621)
Communist$_{t-1}$	-0.962	-0.575
	(0.596)	(0.567)
Civil war$_{t-1}$	-0.783**	-0.762**
	(0.340)	(0.339)
Previous campaign	3.586***	3.744***
	(0.260)	(0.260)
Intercept	-10.50***	-6.614***
	(2.301)	(2.124)
Log-likelihood	-333.63	-321.04
AIC	697.26	676.07
Observations	3,004	3,004
Countries	127	127

Notes: All models are logistic regression models with *Single party* as reference category. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix D

Robustness checks

Table D.1 *Alternative operationalizations and sensitivity analyses for Model 3*

	Democracy included	Imputed data	Higher threshold	Original typology	Cheibub et al.'s (2010) regime data	Prev. camp (alt.1)	Prev. camp. (alt.2)	UCDP/ PRIO
No parties$_{t-1}$	-0.904**	-0.936**	-1.366***		-0.808**	-0.853**	-0.879**	-0.945**
	(0.387)	(0.389)	(0.486)		(0.360)	(0.379)	(0.377)	(0.392)
Multiple parties$_{t-1}$	-0.830**	-1.190***	-1.173***		-0.940**	-0.786**	-0.804**	-0.892**
	(0.412)	(0.378)	(0.454)		(0.382)	(0.372)	(0.373)	(0.404)
Multiparty autocracy$_{t-1}$				-1.037**				
				(0.433)				
Military regime$_{t-1}$				-0.332				
				(0.419)				
Monarchy$_{t-1}$				-1.375				
				(1.044)				
Other autocracy$_{t-1}$				-1.332*				
				(0.686)				
Controls								
Military regime$_{t-1}$	0.571*	0.437	0.582		0.703**	0.572**	0.571**	0.595*
	(0.307)	(0.299)	(0.379)		(0.301)	(0.271)	(0.272)	(0.318)
Ln GDP per capita$_{t-1}$	0.0910	0.201	0.338	0.0971	0.140	0.198	0.190	0.118
	(0.219)	(0.214)	(0.275)	(0.210)	(0.271)	(0.234)	(0.226)	(0.225)
Ln oil and gas income$_{t-1}$	-1.519***	-1.778***	-2.462***	-1.354**	-1.661***	-1.548***	-1.551***	-1.662***
	(0.521)	(0.599)	(0.750)	(0.608)	(0.592)	(0.525)	(0.534)	(0.598)
Urban pop.$_{t-1}$	0.020**	0.017*	0.022**	0.0215**	0.0207*	0.015	0.016*	0.021**
	(0.010)	(0.010)	(0.010)	(0.00975)	(0.0116)	(0.010)	(0.010)	(0.010)
Population$_{t-1}$	0.400***	0.314***	0.258**	0.360***	0.466***	0.378***	0.369***	0.376***
	(0.100)	(0.093)	(0.118)	(0.0890)	(0.114)	(0.081)	(0.079)	(0.098)
Democratic neighbors$_{t-1}$	0.382	1.271**	1.080*	0.590	0.360	0.705	0.711	0.641
	(0.581)	(0.551)	(0.562)	(0.563)	(0.727)	(0.543)	(0.533)	(0.586)
Neighboring campaign$_{t-1}$	0.608**	0.539**	0.749**	0.635**	0.615**	0.477*	0.501*	0.633**
	(0.272)	(0.264)	(0.300)	(0.278)	(0.300)	(0.265)	(0.270)	(0.280)
Ethnic fractionalization	0.727	0.196	1.155	0.663	0.706	0.379	0.376	0.553
	(0.607)	(0.589)	(0.758)	(0.590)	(0.637)	(0.549)	(0.544)	(0.628)
Religious fractionalization	-1.398***	-0.813	-1.242*	-1.291**	-1.567***	-0.901*	-0.911*	-1.368**
	(0.534)	(0.540)	(0.637)	(0.556)	(0.582)	(0.531)	(0.523)	(0.553)
Communist$_{t-1}$	-0.578	-1.251**	-1.050	-0.778	-0.259	-0.480	-0.475	-0.746
	(0.567)	(0.527)	(0.682)	(0.532)	(0.487)	(0.546)	(0.539)	(0.549)
Civil war$_{t-1}$	-0.766**	-0.584*	-0.764**	-0.710**	-0.933**	-0.320	-0.308	
	(0.325)	(0.309)	(0.369)	(0.342)	(0.373)	(0.278)	(0.276)	
Previous campaign	3.313***	3.502***	3.507***	3.365***	3.441***			3.450***
	(0.223)	(0.227)	(0.288)	(0.248)	(0.270)			(0.256)
Democracy$_{t-1}$	-3.965***							
	(0.753)							
Ongoing campaign$_{t-1}$						4.504***	4.541***	
						(0.493)	(0.403)	
Ln time in peace						0.160		
						(0.175)		
Decay function of time in peace							-0.649	
							(0.517)	
Armed conflict$_{t-1}$								-0.962***
								(0.296)
Intercept	-8.436***	-8.290***	-9.593***	-8.062***	-9.535***	-9.543***	-8.885***	-8.407***
	(1.986)	(1.823)	(2.672)	(1.839)	(2.258)	(2.147)	(1.909)	(2.060)
Log-likelihood	-406.65	-403.63	-307.50	-361.72	-324.90	-326.98	-326.77	-358.93
AIC	845.30	837.27	645.01	755.44	679.80	685.95	685.55	747.85
Observations	4,970	3,361	3,010	3,010	2,842	3,010	3,010	3,010
Countries	164	142	127	127	119	127	127	127

Notes: All models are logit models with *Single party* as ref. cat. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1

Table D.2 *Alternative operationalizations and sensitivity analyses for Model 8*

	Democracy included	Imputed data	Higher threshold	Original typology	Cheibub et al.'s (2010) regime data	Prev. camp. (alt.1)	Prev. camp. (alt.2)	UCDP/ PRIO
No parties$_{t-1}$	-0.875**	-0.934**	-1.387***		-0.815**	-0.889**	-0.894**	-0.964**
	(0.366)	(0.384)	(0.486)		(0.341)	(0.368)	(0.366)	(0.375)
Multiple parties$_{t-1}$	-2.961***	-3.171***	-3.123***		-3.274***	-2.707***	-2.682***	-3.179***
	(0.725)	(0.806)	(0.841)		(1.037)	(0.740)	(0.736)	(0.754)
Ln regime duration$_{t-1}$	-0.0326	-0.0334	-0.0867	-0.010	0.117	-0.080	-0.0699	-0.0888
	(0.147)	(0.208)	(0.216)	(0.217)	(0.163)	(0.185)	(0.180)	(0.191)
*Multiparty*regime duration*	0.958***	0.911***	0.877***	1.149***	0.896***	0.825***	0.813***	1.021***
	(0.280)	(0.310)	(0.329)	(0.363)	(0.339)	(0.293)	(0.293)	(0.298)
Multiparty autocracy$_{t-1}$				-3.378***				
				(0.913)				
Military regime$_{t-1}$				-0.350				
				(0.415)				
Monarchy$_{t-1}$				-1.310				
				(1.091)				
Other autocracy$_{t-1}$				-1.447*				
				(0.764)				
Controls								
Military regime$_{t-1}$	0.340	0.340	0.511		0.732**	0.529*	0.525*	0.515
	(0.301)	(0.301)	(0.384)		(0.302)	(0.286)	(0.286)	(0.315)
Ln GDP per capita$_{t-1}$	0.100	0.100	0.203	-0.137	0.101	0.0706	0.0699	-0.0171
	(0.235)	(0.235)	(0.272)	(0.232)	(0.268)	(0.237)	(0.234)	(0.238)
Ln oil and gas income$_{t-1}$	-1.468***	-1.468***	-2.204***	-1.286*	-1.552***	-1.447**	-1.453**	-1.410**
	(0.534)	(0.534)	(0.767)	(0.733)	(0.596)	(0.578)	(0.586)	(0.603)
Urban pop.$_{t-1}$	0.0142	0.0142	0.0190*	0.0212*	0.0178	0.0154	0.0156	0.0178
	(0.0106)	(0.0106)	(0.0105)	(0.0112)	(0.0118)	(0.0110)	(0.0107)	(0.0113)
Population$_{t-1}$	0.267***	0.267***	0.193	0.312***	0.377***	0.356***	0.349***	0.321***
	(0.0917)	(0.0917)	(0.120)	(0.0926)	(0.106)	(0.0872)	(0.0863)	(0.103)
Democratic neighbors$_{t-1}$	1.501***	1.501***	1.307**	0.905	0.494	0.937	0.935	0.819
	(0.558)	(0.558)	(0.631)	(0.606)	(0.727)	(0.576)	(0.570)	(0.642)
Neighboring campaign$_{t-1}$	0.629**	0.629**	0.825***	0.725***	0.576*	0.531**	0.547**	0.707**
	(0.267)	(0.267)	(0.293)	(0.278)	(0.317)	(0.261)	(0.265)	(0.282)
Ethnic fractionalization	0.0525	0.0525	0.934	0.541	0.677	0.244	0.240	0.333
	(0.566)	(0.566)	(0.711)	(0.537)	(0.621)	(0.546)	(0.546)	(0.575)
Religious fractionalization	-0.533	-0.533	-0.994	-1.397***	-1.159*	-0.819	-0.819	-1.047*
	(0.557)	(0.557)	(0.629)	(0.483)	(0.614)	(0.548)	(0.544)	(0.575)
Communist$_{t-1}$	-1.176**	-1.176**	-0.927	-0.654	-0.532	-0.387	-0.387	-0.639
	(0.544)	(0.544)	(0.708)	(0.571)	(0.492)	(0.575)	(0.573)	(0.565)
Civil war$_{t-1}$	-0.490	-0.490	-0.645*	-0.581	-0.813**	-0.233	-0.228	
	(0.313)	(0.313)	(0.376)	(0.375)	(0.380)	(0.281)	(0.279)	
Previous campaign	3.641***	3.502***	3.608***	3.476***	3.615***			3.571***
	(0.241)	(0.227)	(0.297)	(0.257)	(0.280)			(0.265)
Democracy$_{t-1}$	-3.882***							
	(0.754)							
Ongoing campaign$_{t-1}$						4.317***	4.373***	
						(0.485)	(0.375)	
Ln time in peace						0.0920		
						(0.178)		
Decay function of time in peace							-0.422	
							(0.501)	
Armed conflict$_{t-1}$								-0.874***
								(0.317)
Intercept	-7.044***	-7.074***	-7.750***	-5.872***	-8.766***	-8.001***	-7.654***	-6.646***
	(1.944)	(1.878)	(2.482)	(1.800)	(2.133)	(2.025)	(1.914)	(1.991)
Log-likelihood	-391.75	-403.63	-297.25	-344.40	-311.93	-319.85	-319.72	-343.95
AIC	819.51	837.27	628.49	724.80	657.85	675.70	675.45	721.90
Observations	4,970	3,361	3,010	3,010	2,842	3,010	3,010	3,010
Countries	164	142	127	127	119	127	127	127

Notes: All models are logistic regression models with *Single party* as reference category. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.3 *Alternative estimations for Model 3*

	Probit	Multinomial logit		
		Nonviolent	Violent	Both
No parties$_{t-1}$	-0.430**	-0.801*	-0.367	-1.198
	(0.187)	(0.424)	(0.297)	(0.891)
Multiple parties$_{t-1}$	-0.426**	-0.991**	-0.435	-0.823
	(0.185)	(0.464)	(0.331)	(1.048)
Controls				
Military regime$_{t-1}$	0.261*	0.531	0.346	1.144
	(0.144)	(0.339)	(0.249)	(0.793)
Ln GDP per capita$_{t-1}$	0.0938	0.222	0.0303	-0.774
	(0.107)	(0.260)	(0.160)	(0.787)
Ln oil and gas income$_{t-1}$	-0.767***	-1.455***	-0.294	-5.555*
	(0.277)	(0.516)	(0.280)	(3.006)
Urban population$_{t-1}$	0.008*	0.0227**	-0.0108	0.0247
	(0.005)	(0.0100)	(0.00810)	(0.0257)
Population$_{t-1}$	0.179***	0.369***	0.281***	0.963***
	(0.046)	(0.0993)	(0.0912)	(0.317)
Democratic neighbors$_{t-1}$	0.355	1.088*	0.560	-3.513
	(0.275)	(0.589)	(0.497)	(2.872)
Neighboring campaign$_{t-1}$	0.281**	0.428	0.102	1.088
	(0.125)	(0.312)	(0.251)	(0.840)
Ethnic fractionalization	0.194	1.045	0.126	-0.835
	(0.281)	(0.702)	(0.558)	(0.913)
Religious fractionalization	-0.471*	-1.008*	-0.782	-5.639***
	(0.261)	(0.543)	(0.483)	(2.080)
Communist$_{t-1}$	-0.328	-0.627	-1.046	-13.79***
	(0.260)	(0.599)	(0.837)	(1.198)
Civil war$_{t-1}$	-0.327**	0.850	6.022***	17.95***
	(0.146)	(0.561)	(0.271)	(0.454)
Previous campaign	1.797***	3.665***	1.523***	3.982***
	(0.136)	(0.307)	(0.472)	(0.796)
Intercept	-4.469***	-9.756***	-5.812***	-21.47***
	(0.937)	(2.488)	(1.511)	(7.670)
Log-likelihood	-362.34		-739.60	
AIC	754.68		1569.20	
Observations	3,010		3,010	
Countries	127		127	

Notes: All models are logistic regression models with *Single party* as reference category. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.4 *Alternative estimations for Model 8*

	Probit	Multinomial logit		
		Nonviolent	Violent	Both
No parties$_{t-1}$	-0.436**	-0.709*	-0.302	-1.906*
	(0.175)	(0.395)	(0.341)	(1.145)
Multiple parties$_{t-1}$	-1.599***	-3.021***	-0.0394	-4.864**
	(0.377)	(0.789)	(0.670)	(1.946)
Ln regime duration$_{t-1}$	-0.0324	0.0560	0.0989	-0.736*
	(0.0890)	(0.203)	(0.194)	(0.389)
*Multiparty*regime duration*	0.516***	0.930***	-0.182	1.702***
	(0.152)	(0.316)	(0.235)	(0.596)
Controls				
Military regime$_{t-1}$	0.210	0.459	0.370	1.158
	(0.144)	(0.315)	(0.250)	(0.767)
Ln GDP per capita$_{t-1}$	0.0269	0.0742	0.0446	-1.222
	(0.117)	(0.262)	(0.161)	(0.788)
Ln oil and gas income$_{t-1}$	-0.696**	-1.190**	-0.317	-8.048**
	(0.301)	(0.519)	(0.298)	(3.761)
Urban population$_{t-1}$	0.00719	0.0188	-0.0115	0.0367
	(0.00546)	(0.0115)	(0.00781)	(0.0305)
Population$_{t-1}$	0.165***	0.301***	0.280***	1.320***
	(0.0478)	(0.101)	(0.0958)	(0.400)
Democratic neighbors$_{t-1}$	0.461	1.364**	0.566	-4.315
	(0.302)	(0.634)	(0.499)	(2.744)
Neighboring campaign$_{t-1}$	0.329***	0.524*	0.0824	0.903
	(0.127)	(0.315)	(0.255)	(0.914)
Ethnic fractionalization	0.127	0.820	0.145	-0.430
	(0.270)	(0.634)	(0.545)	(1.037)
Religious fractionalization	-0.382	-0.738	-0.775	-6.280***
	(0.276)	(0.566)	(0.488)	(1.986)
Communist$_{t-1}$	-0.301	-0.577	-1.054	-14.21***
	(0.268)	(0.626)	(0.835)	(1.607)
Civil war$_{t-1}$	-0.286**	0.967*	6.044***	18.76***
	(0.145)	(0.567)	(0.277)	(0.501)
Previous campaign	1.857***	3.829***	1.545***	4.241***
	(0.137)	(0.319)	(0.485)	(0.879)
Intercept	-3.755***	-8.157***	-6.155***	-20.84***
	(0.938)	(2.342)	(1.527)	(7.229)
Log-likelihood	-403.63		-721.67	
AIC	837.27		1545.34	
Observations	3,361		3,010	
Countries	127		127	

Notes: All models are logistic regression models with *Single party* as reference category. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.5 *Investigating the effects of omitted variable bias, Model 3*

	Random effects	Glob. camp. average	Executive tenure	Repression (PTS)	Welfare spending
No parties$_{t-1}$	-1.614***	-0.919**	-0.811**	-1.044**	-1.072***
	(0.489)	(0.394)	(0.401)	(0.432)	(0.399)
Multiple parties$_{t-1}$	-1.413***	-0.933**	-0.861**	-0.941**	-0.853**
	(0.517)	(0.391)	(0.418)	(0.430)	(0.406)
Controls					
Military regime$_{t-1}$	0.935**	0.610**	0.605*	0.659**	0.634**
	(0.400)	(0.300)	(0.311)	(0.331)	(0.314)
Ln GDP per capita$_{t-1}$	0.139	0.130	0.129	0.260	0.0621
	(0.293)	(0.224)	(0.233)	(0.264)	(0.227)
Ln oil and gas income$_{t-1}$	-1.797**	-1.687***	-1.719***	-1.644**	-1.505**
	(0.835)	(0.615)	(0.603)	(0.668)	(0.589)
Urban population$_{t-1}$	0.0224*	0.0224**	0.0215**	0.0178*	0.0254**
	(0.0131)	(0.00984)	(0.0100)	(0.0107)	(0.00997)
Population$_{t-1}$	0.502***	0.380***	0.378***	0.294**	0.327***
	(0.141)	(0.105)	(0.103)	(0.128)	(0.100)
Democratic neighbors$_{t-1}$	1.274*	0.611	0.678	0.751	0.571
	(0.749)	(0.570)	(0.576)	(0.574)	(0.603)
Neighboring campaign$_{t-1}$	0.662**	0.519*	0.602**	0.552*	0.669**
	(0.327)	(0.309)	(0.284)	(0.287)	(0.287)
Ethnic fractionalization	0.638	0.505	0.506	0.884	0.626
	(0.773)	(0.609)	(0.637)	(0.711)	(0.650)
Religious fractionalization	-1.156*	-1.185**	-1.170**	-1.270**	-1.145*
	(0.685)	(0.555)	(0.576)	(0.642)	(0.607)
Communist$_{t-1}$	-0.322	-0.714	-0.746	-0.747	-0.563
	(0.710)	(0.551)	(0.557)	(0.792)	(0.571)
Civil war$_{t-1}$	-0.564	-0.748**	-0.763**	-0.752**	-0.859**
	(0.371)	(0.316)	(0.325)	(0.380)	(0.354)
Previous campaign	3.075***	3.392***	3.496***	3.526***	3.577***
	(0.300)	(0.254)	(0.247)	(0.281)	(0.249)
Nonviolent campaign average, globally$_{t-1}$		4.636			
		(6.358)			
Ln executive tenure$_{t-1}$			0.148		
			(0.122)		
Repression$_{t-1}$				0.267**	
				(0.118)	
Ln welfare spending$_{t-1}$					-0.0297
					(0.0405)
Intercept	-10.18***	-9.756***	-5.812***	-9.399***	-7.696***
	(2.714)	(2.488)	(1.511)	(2.205)	(1.977)
Log-likelihood	-354.25	-360.49	-360.03	-317.39	-335.23
AIC	740.50	752.98	752.06	666.79	702.47
Observations	3,010	3,010	3,010	2,479	2,770
Countries	127	127	127	125	123

Notes: All models are logistic regression models with *Single party* as reference category. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.5 *Investigating the effects of omitted variable bias, Model 3* **(continued)**

	Growth	**Cold war**	**FH CL**	**SIP**
No parties$_{t-1}$	-0.966**	-0.924**	-1.215***	-0.785*
	(0.399)	(0.389)	(0.420)	(0.437)
Multiple parties$_{t-1}$	-0.970**	-0.861**	-1.178***	-0.838**
	(0.404)	(0.420)	(0.416)	(0.396)
Controls				
Military regime$_{t-1}$	0.589*	0.445	0.715**	0.421
	(0.307)	(0.308)	(0.322)	(0.355)
Ln GDP per capita$_{t-1}$	0.129	0.0421	0.103	0.131
	(0.223)	(0.238)	(0.226)	(0.308)
Ln oil and gas income$_{t-1}$	-1.694***	-1.582***	-1.539**	-1.657***
	(0.606)	(0.585)	(0.630)	(0.621)
Urban population$_{t-1}$	0.0218**	0.0237**	0.0210**	0.0232**
	(0.00974)	(0.0100)	(0.00971)	(0.0113)
Population$_{t-1}$	0.371***	0.389***	0.344***	0.357***
	(0.0995)	(0.0961)	(0.0998)	(0.107)
Democratic neighbors$_{t-1}$	0.667	0.881	0.636	1.054*
	(0.572)	(0.580)	(0.570)	(0.611)
Neighboring campaign$_{t-1}$	0.612**	0.680**	0.561**	0.576*
	(0.279)	(0.297)	(0.279)	(0.299)
Ethnic fractionalization	0.568	0.680	0.591	0.270
	(0.610)	(0.608)	(0.616)	(0.722)
Religious fractionalization	-1.182**	-1.087*	-1.100*	-0.732
	(0.560)	(0.556)	(0.565)	(0.554)
Communist$_{t-1}$	-0.746	-0.842	-0.556	-1.055*
	(0.557)	(0.554)	(0.584)	(0.560)
Civil war$_{t-1}$	-0.737**	-0.763**	-0.690**	-0.557
	(0.318)	(0.315)	(0.330)	(0.343)
Previous campaign	3.433***	3.422***	3.430***	3.544***
	(0.246)	(0.254)	(0.256)	(0.274)
GDP Growth	-0.0230			
	(0.809)			
Cold War		0.448		
		(0.321)		
FH Civil Liberties$_{t-1}$			-0.182	
			(0.151)	
Cen. SIP score$_{t-1}$				-2.461**
				(1.148)
SIP score squared$_{t-1}$				-3.893
				(3.070)
Intercept	-8.543***	-8.554***	-7.086***	-8.788***
	(2.006)	(1.944)	(2.408)	(2.754)
Log-likelihood	-361.03	-359.49	-346.73	-332.98
AIC	754.07	750.98	725.45	699.96
Observations	3,010	3,010	2,916	2,721
Countries	127	127	127	121

Notes: All models are logistic regression models with Single party as reference category. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.6 *Investigating the effects of omitted variable bias, Model 8*

	Random effects	**Glob. camp. average**	**Executive tenure**	**Repression (PTS)**	**Welfare spending**
No parties$_{t-1}$	-1.798***	-1.005***	-1.007***	-1.162***	-1.114***
	(0.464)	(0.384)	(0.386)	(0.424)	(0.384)
Multiple parties$_{t-1}$	-4.770***	-3.348***	-3.329***	-3.431***	-3.296***
	(1.024)	(0.755)	(0.771)	(0.748)	(0.803)
Ln regime duration$_{t-1}$	0.0822	-0.139	-0.114		-0.0809
	(0.245)	(0.194)	(0.195)	-0.189	(0.205)
*Multiparty*regime duration*	1.523***	1.063***	1.049***	(0.186)	1.105***
	(0.411)	(0.303)	(0.302)	1.078***	(0.318)
Controls				(0.289)	
Military regime$_{t-1}$	0.520	0.531*	0.508	0.561*	0.529*
	(0.408)	(0.305)	(0.309)	(0.337)	(0.306)
Ln GDP per capita$_{t-1}$	-0.153	-0.00944	-0.0102	0.0866	-0.133
	(0.347)	(0.241)	(0.238)	(0.270)	(0.231)
Ln oil and gas income$_{t-1}$	-1.802*	-1.432**	-1.444**	-1.353**	-1.165**
	(1.040)	(0.607)	(0.607)	(0.646)	(0.574)
Urban population$_{t-1}$	0.0243	0.0197*	0.0189*	0.0165	0.0217*
	(0.0161)	(0.0114)	(0.0113)	(0.0119)	(0.0115)
Population$_{t-1}$	0.581***	0.331***	0.322***	0.256*	0.284***
	(0.178)	(0.109)	(0.105)	(0.136)	(0.106)
Democratic neighbors$_{t-1}$	1.801**	0.820	0.872	1.032	0.720
	(0.824)	(0.633)	(0.630)	(0.630)	(0.657)
Neighboring campaign$_{t-1}$	0.809**	0.614**	0.691**	0.651**	0.786***
	(0.361)	(0.296)	(0.281)	(0.278)	(0.285)
Ethnic fractionalization	0.215	0.322	0.370	0.736	0.384
	(0.893)	(0.569)	(0.572)	(0.683)	(0.583)
Religious fractionalization	-1.239	-0.908	-0.897	-0.931	-0.826
	(0.830)	(0.573)	(0.576)	(0.658)	(0.605)
Communist$_{t-1}$	-0.616	-0.596	-0.634	-0.611	-0.467
	(0.736)	(0.577)	(0.573)	(0.814)	(0.582)
Civil war$_{t-1}$	-0.500	-0.636**	-0.626*	-0.628*	-0.765**
	(0.405)	(0.320)	(0.320)	(0.381)	(0.362)
Previous campaign	2.949***	3.519***	3.558***	3.612***	3.730***
	(0.337)	(0.262)	(0.254)	(0.288)	(0.264)
Nonviolent campaign average, globally$_{t-1}$		3.940			
		(6.000)			
Ln executive tenure$_{t-1}$			0.013		
			(0.120)		
Repression$_{t-1}$				0.085	
				(0.143)	
Ln welfare spending$_{t-1}$					0.002
					(0.035)
Intercept	-8.842***	-6.931***	-6.792***	-6.994***	-5.843***
	(2.989)	(2.016)	(1.957)	(2.176)	(1.855)
Log-likelihood	-354.25	-360.49	-360.03	-303.88	-318.59
AIC	740.50	752.98	752.06	643.77	673.19
Observations	3,010	3,010	3,010	2,479	2,770
Countries	127	127	127	125	123

Notes: All models are logistic regression models with Single party as reference category. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D.6 *Investigating the effects of omitted variable bias, Model 8* **(continued)**

	GDP Growth	Cold war	FH CL	SIP
No parties$_{t-1}$	-1.006***	-0.902**	-1.261***	-0.847**
	(0.386)	(0.372)	(0.396)	(0.416)
Multiple parties$_{t-1}$	-3.332***	-3.095***	-3.668***	-3.027***
	(0.757)	(0.815)	(0.783)	(0.858)
Ln regime duration$_{t-1}$	-0.106	-0.0378	-0.0383	-0.142
	(0.194)	(0.196)	(0.202)	(0.206)
*Multiparty*regime duration*	1.051***	1.011***	1.078***	0.944***
	(0.301)	(0.314)	(0.310)	(0.315)
Controls				
Military regime$_{t-1}$	0.500	0.335	0.641**	0.418
	(0.310)	(0.321)	(0.317)	(0.350)
Ln GDP per capita$_{t-1}$	-0.0181	-0.107	-0.0503	0.00782
	(0.239)	(0.251)	(0.238)	(0.314)
Ln oil and gas income$_{t-1}$	-1.478**	-1.335**	-1.228**	-1.452**
	(0.601)	(0.574)	(0.610)	(0.642)
Urban population$_{t-1}$	0.0192*	0.0210*	0.0172	0.0205*
	(0.0112)	(0.0113)	(0.0114)	(0.0119)
Population$_{t-1}$	0.326***	0.335***	0.282***	0.313***
	(0.102)	(0.101)	(0.104)	(0.109)
Democratic neighbors$_{t-1}$	0.868	1.076*	0.813	1.081*
	(0.631)	(0.626)	(0.624)	(0.617)
Neighboring campaign$_{t-1}$	0.693**	0.761***	0.639**	0.630**
	(0.280)	(0.294)	(0.285)	(0.299)
Ethnic fractionalization	0.361	0.513	0.370	0.121
	(0.560)	(0.556)	(0.555)	(0.663)
Religious fractionalization	-0.909	-0.789	-0.743	-0.549
	(0.578)	(0.563)	(0.575)	(0.601)
Communist$_{t-1}$	-0.633	-0.756	-0.377	-0.835
	(0.578)	(0.578)	(0.599)	(0.574)
Civil war$_{t-1}$	-0.626**	-0.652**	-0.550*	-0.459
	(0.319)	(0.322)	(0.319)	(0.345)
Previous campaign	3.546***	3.550***	3.549***	3.630***
	(0.256)	(0.268)	(0.269)	(0.273)
GDP Growth	-0.451			
	(0.655)			
Cold War		0.504*		
		(0.297)		
FH Civil Liberties$_{t-1}$			-0.273**	
			(0.134)	
Cen. SIP score$_{t-1}$				-1.485
				(1.098)
SIP score squared$_{t-1}$				-1.832
				(2.903)
Intercept	-6.758***	-6.920***	-4.753**	-7.034***
	(1.943)	(1.874)	(2.213)	(2.665)
Log-likelihood	-345.69	-343.99	-329.14	-323.92
AIC	727.39	723.98	694.29	685.83
Observations	3,010	3,010	2,916	2,721
Countries	127	127	127	121

Notes: All models are logistic regression models with Single party as reference category. Territorial campaigns are excluded and none of the models include region or decade dummies. Clustered robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1